

SCIENCE

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FRIDAY, MARCH 20, 1896.

VIVISECTION.*

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A STATEMENT IN BEHALF OF SCIENCE.

So long ago as the autumn of 1866 there were published in New York denunciations of the practice of making upon living animals those scientific observations and ex-

* The sciences which have to do with animal experimentation are physiology, physiological chemistry, pharmacology, medical chemistry, toxicology, morphology (including anatomy and embryology), bacteriology, pathology, medicine and surgery. These sciences are largely represented in this country by the American Physiological Society, the American Society of Morphologists, the American Anatomical Society, the American Society of Naturalists, the American Society of Physicians, and the American Society of Surgeons.

In December last the presidents of the above societies were invited to appoint members of a joint committee to sit in Philadelphia on the occasion of the annual meeting in that city of several of these associations.

The accompanying 'statement in behalf of science' was adopted by this joint committee of thirty-four members, and is now published over their signatures, with the addition of several names of persons specially qualified to speak on the subject, but not members of the committee. It sets forth the importance of animal experimentation for the advancement of medicine, and may be accepted as an authoritative expression of expert opinion on this question.

(Signed) CHARLES W. ELIOT,
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FRANCIS A. WALKER,
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President of the Massachusetts Medical Society.

BOSTON, February 24, 1896.

periments which are commonly called vivisections. During the following twenty-nine years there have appeared, from time to time, at one or another place, similar denunciations, more or less sweeping and violent. Of these some condemn vivisection altogether, and others in various of its phases. Some call for its total abolition, and others for its material restriction. Some are labored essays, and others are brief 'tracts' or 'leaflets' intended more easily to arrest the attention. Most of these publications, however, have this in common, that they seek to fortify argument with strenuous appeals to emotion; and in some the tone of invective rises to a shrillness little short of frantic. In these publications, too, there often figure extracts from scientific writings; and, in many cases, these extracts are so garbled that only ignorant or reckless animosity could be accepted in excuse for their seeming bad faith.

During the past twenty-nine years these attacks have but little disturbed the calm of biology and medicine in this country; but, from time to time, it has seemed wise to take some notice of them, inasmuch as the common sense of some members of a changing community is liable to be led astray as to a subject which is largely technical in its nature. The following statement, therefore, is added to its predecessors. Its signers, however, are well aware that they can hardly hope to make any statement or to draw any conclusion which some anti-vivisectionist agitator will not promptly denounce as false or immoral.

Science is simply common knowledge made precise, extended and transmitted from generation to generation of trained observers and reasoners. The biological sciences study in the most varied ways the bodies and the lives of men, of animals and of plants. The applied sciences utilize knowledge thus obtained for the every-day good of mankind; and one of these applied

sciences, medicine, brings biological discoveries to bear upon the prevention and cure of disease and injury. As experience grows incessantly, the fact which has laboriously been established with no other thought than the noble one of advancing knowledge may be applied, the next day or the next century, in the most practical way by some inventor or physician; and, in the application, new facts may come to light, which will markedly extend the boundaries of knowledge.

Therefore, in the slowly woven fabric of achievement, pure science and applied science, biology and medicine, have always been warp and woof. Let either be destroyed, man's life shall go threadbare.

To show this, a few out of many striking examples may suffice.

Not very long ago the red clover was imported into a British colony to which it was not native. The plant thrived, when planted; but its flowers set no seeds, so that fresh seed had to be brought from the mother country. The disappointed farmers consulted people who had given up their time to the study of plants and insects—botanists, and 'bug-hunters,' in fact. Pure science told the practical farmers that the long-billed humble-bees which sucked honey in every English clover field also carried pollen from flower to flower, and thus fertilized the plants, and that it was useless to try for crops of imported red clover, unless humble-bees were imported also.

No less enlightening is the history of one of the latest and most modern of the developments of science. Near the end of the last century Dr. Galvani, an Italian professor of anatomy, set himself to investigate the cause of a newly discovered fact: namely, that the muscles of the legs of freshly killed frogs jerked forcibly when their nerves were worked upon by the taking of a spark from an electrical machine. This investigation, which does not sound

momentous, he undertook, 'in order to discover the hidden properties' of the nerves and muscles, 'and to treat their diseases more certainly.' To the jerks of Galvani's frogs' legs we owe the discovery of the galvanic battery and current, which are named after him; the telegraph and ocean cable, with their immense influence upon civilized life in peace and war; the transfer to miles of distance of the vast working power of Niagara Falls. It is a fitting, if slight, dramatic touch that the traveller in Italy who passes the night at Bologna, where Galvani worked and taught, will perhaps put up at a hotel directly opposite the professor's modest house, and will see that the tablet which records the experiments made within is lighted up at evening by the electric light, which also owes its existence to a search for the hidden 'properties' of frogs' legs.

Two hundred years ago there lived at Delft, in Holland, a well-to-do Dutchman, named Antony van Leeuwenhoek. He had been a 'dry goods clerk' in his youth, and had had no learned or professional training. Van Leeuwenhoek took to making and polishing, for his own use, very small and very strong magnifying glasses, because he was full of what some anti-vivisectionists sneer at as 'scientific curiosity.' The Dutchman's glasses were very superior; and with them he looked at the most miscellaneous things—among these, at ditch water and at particles from the surface of his own teeth. He found that such matters were swarming with living things of all kinds, and described them and other things so well that he became famous, and princes, who were not ashamed to be interested in 'mere science,' sent for him and his glasses to instruct them. Among Van Leeuwenhoek's discoveries were the minute things now called bacteria, or microbes, and known to be living plants. The physicians were prompted to guess that diseases might be

due to the ravages of the new forms of microscopic life first seen with decisive clearness by Van Leeuwenhoek; but no proof of this was forthcoming, and the idea was abandoned by most, amid the laughter of many at this fad of the doctors. More than a century went by. The bacteria, as objects of pure science, were more and more studied. The microscope was bettered more and more from the simple magnifying glass of Van Leeuwenhoek. With the advance of chemistry and of other sciences, all known means of studying minute living things became greatly improved; and now the idea that many diseases were caused by minute living things was taken up afresh, and carried to triumphant demonstration by a number of medical men and biologists—among the latter by Pasteur, whose recent loss is mourned by the world, and whom an eminent American humanitarian sneered at, not many years ago, as an 'obscure druggist.' The proof that many diseases are caused each by a particular kind of microbe was obtained by vivisection; for the proof consisted in inoculating animals with the special microbe in question, to the practical exclusion of others, and noting that the animals took the disease, perhaps died of it. As some only of the results of the knowledge thus gained by experiment upon animals, it may be noted that the prevention of cholera has been made more certain, and that great numbers of patients, largely children, have been saved from death by the anti-toxine treatment of diphtheria. But every child thus saved to-day owes his life, not only to medicine, but to biology; not only to the observations and the vivisections of Klebs and Loeffler and Koch and Pasteur and others, but to the 'mere scientific curiosity' of that old lens-polisher of Delft, who spent time in prying into ditch water and particles from the surface of teeth.

Early in the last century, at a country

parsonage in England, there worked a pious and gifted man, the Rev. Stephen Hales, D. D., Rector of Farringdon, in Hampshire. Dr. Hales achieved the uncommon distinction of becoming both an excellent clergyman and a famous biologist. Nor was it to any easy branch of observation that he gave such time as he could spare, but to difficult themes of experimental physiology, both vegetable and animal. He studied, among other things, the pressure of the sap in plants and the pressure of the blood in the vessels of animals. In order to investigate the blood pressure, he did a number of indispensable vivisections upon horses, sheep and dogs. Each animal was tied down, an artery was opened and connected with a pressure gauge, and the true pressures and their variations were for the first time properly observed and recorded. No doubt, had it been possible, the excellent Hales would have drugged his animals to quiet their pain; but modern methods for this purpose were not discovered till long afterward, so that in those days both man and beast faced the surgeon's knife without such relief as they afford. By the work of Hales our knowledge of the circulation of the blood, which his famous compatriot Harvey had discovered, received an essential addition; nor is there reason to suppose that Hales ever doubted the morality of the proceedings by which he satisfied his 'scientific curiosity.' Were he to return to life and to repeat his experiments, even with all modern improvements, he certainly would be surprised at the reception he would meet with in some quarters.

Since the time of Hales those changes in the blood pressure have carefully been studied which are produced in various states of the system and by various drugs. More than a century after Hales some vivisections were performed by Mr. Arthur Gamgee, to test the effect upon the blood

pressure of a certain volatile chemical—the nitrite of amyl. It was found that the pressure appeared to be greatly lessened by this drug. Some of these experiments were witnessed by Dr. T. Lauder Brunton, at that time resident physician to the Royal Infirmary at Edinburgh, and now an eminent medical practitioner and professor in London. During the winter of 1866–67 there were in the wards of the infirmary several patients who suffered from the disorder called breastpang, or angina pectoris, which is characterized by paroxysms of hard breathing and of terrible pain over the heart. In observing these cases, Dr. Brunton saw reason to think that the attack was accompanied by a high blood pressure in the arteries. He remembered the vivisections in which he had seen the effects upon the arterial pressure of the nitrite of amyl. He caused his patients to inhale a few drops of the volatile drug. The pain generally disappeared; and the nitrite of amyl became very soon a recognized agent for the relief of one of the most acute forms of human suffering.

Every victim of angina who carries this drug about with him for use at any moment owes his exemption, first, to the scientific physician; second, to the pharmacologist—that is, the scientific student of the action of drugs, who, for the good of man, sacrificed animals in studying the effect of drugs upon the blood pressure; and third, to the clergyman and physiologist, Hales, who a century before had given some pain to animals in studying the science of the circulation, apart from any direct application to the cure of human ailments. Nor is this all; for the experiments of Hales were based upon the knowledge acquired through vivisection by the physician Harvey, who by this means settled much relating to the motions of the heart and blood in animals; which settlement, in turn, depended upon the work of the famous Greek physician,

Galen, who seventeen centuries ago proved by vivisections, against his professional opponents, that blood is naturally contained in the arteries.

Of the numerous improvements in practical medicine and surgery which are the outcome of experiments upon living animals we could not speak at length without expanding a brief statement into a book. We will instance further only the vivisections by which, at the time of the Napoleonic wars, Dr. J. F. D. Jones ascertained the proper way to tie up a wounded artery, and thereby afforded the means to military and civil practice of saving very numerous patients from bleeding to death; the experiments of the still living surgeon, Sir Joseph Lister, as the result of which surgery has been revolutionized in our own day; the quite recent vivisections, as the result of which the cure of the disease called myxœdema has been discovered, which cure consists in the administration or transplantation of the thyroid gland; and the vivisections in the seventeenth century relating to the transfusion of blood, as the result of which women in child-bed have repeatedly been rescued from impending death from 'flooding after delivery.'

Experience shows, therefore, that it is impossible to disentangle pure science from applied science; that vital human interests are benefited by 'scientific curiosity,' as well as by work more directly practical; and that this general law holds good for those sciences, pure and applied, which deal with man as such, and with the other living things upon the earth. Without physiology, pathology and their allies, which investigate the laws of life by experiments upon living creatures, practical medicine would be in worse than mediæval plight; for before the Middle Ages the genius of the Greeks had inaugurated the practice of experimental physiology, with results of value for all time.

Therefore, the use of animals by mankind for scientific purposes take its place beside those other uses of them for the good of man which involve imprisonment, enforced labor, death, and, in some cases, suffering. That society asserts with practical unanimity the right to kill and inflict pain upon animals for its own purposes is shown by the legal view of cruelty as the unjustifiable infliction of suffering. Were every infliction of pain as such punishable as cruel, the painful operations, for instance, required to make animals docile, or to fit them to be food, would be abolished. In every great civilized country these operations of the farmyard aggregate millions in each year.

Happily, of the very various procedures known collectively as vivisections, many are painless; in others the suffering is trivial, whether the animal be killed or remain alive; and in the great majority of the rest some drug may be given to quiet pain, or insensibility may be produced by sudden operation. There remains, however, a limited portion of cases, which may be of great importance, where the results of experiment would be endangered by any means that could be taken against suffering. In these cases the animal must suffer, though often far less than would be supposed, for the benefit of man as does the gelded horse or the wounded game.

Common sense requires, therefore, that investigations in biology and medicine shall proceed, at the expense, when necessary, of the death and suffering of animals. If these sciences are not to be extinguished they must be transmitted from generation to generation; they must be taught, and like all the other natural or physical sciences; they must, at institutions of the higher learning be taught by demonstration. No one would think favorably of a student of chemistry who had never handled a test-tube, or of a student of electricity who had

never set up a battery. The young astronomer sees the stars and planets themselves through the telescope. So do serious students of biology or medicine see for themselves the structure of the body, see for themselves the workings of that structure through the experiments of the physiological or pathological laboratory or lecture room, just as medical students, they see disease in the wards of hospitals, and look on or assist at the surgical operations performed upon men, women and children. No models and pictures can replace such teaching. From this last fact there is no escape. It is rooted in the constitution of the human mind. No mother would knowingly allow her children to ride behind a locomotive engineer who had never seen the workings of an actual engine. Surely the physician who does his best to guide the living mechanism along the path of safety should be taught its natural workings as exactly and as fully as possible; otherwise he may understand its working in disease.

Happily the cases where the animals seen at demonstrations must undergo more than brief or trivial pain are even rarer than in cases of pure research. In the very great majority of demonstrations the creatures can be kept free of pain until they are killed. As to whether or no, under given circumstances of research or teaching, an experiment involving pain should be performed, is a matter which should rest with the responsible expert, by whom or under whose direction the thing would be done. Otherwise, in a matter involving the interest of the community, those who know would be directed by those who do not know. For any experiment improperly conducted the person responsible is liable under the general laws against the maltreatment of animals. In fact, American biologists and physicians are no more inclined than other members of the community to culpable negligence toward their

fellow-creatures. The work of science goes on; but those who are responsible desire, and see to it, that the work be painless, so far as admissible. No intelligent man or woman should give heed to the denunciations of those few ill-informed or headstrong persons who have been drawn into one of the less wise of the agitations which beset modern society.

Signed: S. Weir Mitchell, J. G. Curtis, W. H. Howell, H. P. Bowditch, W. T. Porter, J. W. Warren, R. H. Chittenden, V. C. Vaughan, John Marshall, S. B. Ward, William Pepper, S. C. Busey, Henry M. Lyman, E. G. Janeway, Ch. Wardell Stiles, William Patten, William T. Sedgwick, H. C. Ernst, Theobald Smith, A. C. Abbott, J. J. Abel, A. R. Cushny, H. C. Wood, Frank Baker, Harrison Allen, G. A. Piersol, C. S. Minot, Henry F. Osborn, C. O. Whitman, William H. Welch, T. M. Prudden, R. H. Fitz, George M. Sternberg, J. Rufus Tryon, Walter J. Wyman, Daniel E. Salmon, G. Brown Goode, W. W. Keen, William Osler, J. Collins Warren, W. T. Councilman.

CERTITUDES AND ILLUSIONS: AN ILLUSION CONCERNING REST.

TWENTY centuries of investigation have dispelled many illusions. In examining the folklore of the world it is found that the lower the stage of culture the greater the number of these illusions. Since systematic researches were inaugurated by the Greeks many have been explained, yet some remain, even in the scientific world of today. On the threshold of our work it becomes necessary to dispel an illusion developed by primordial men and handed down through sequent generations to the present time, so that even now there are few minds unclouded by its mystic presence. When the ball is in the hand it seems to be at rest; when it flies from the hand motion seems to be created; and when it stops upon the

ground motion seems to be destroyed. When the horse stands he seems to be at rest; when he moves motion seems to be created; and when he stops motion seems to be destroyed. The ship is idle in the harbor, and it seems to rest or to be without motion; the winds fill its sail, and it seems that motion is created; it is becalmed at sea and the motion seems to be destroyed. Without the consideration of other unseen facts, rest seems to be a state without motion, and it appears that motion can be created and destroyed. This is the illusion to be dispelled. It is proposed to demonstrate that acceleration in molar motion is deflection of molecular motion, and in general that acceleration in any body is deflection in the particles of the body.

For this purpose it becomes necessary to define what is here meant by the terms body and particle. The universe is discovered to be a hierarchy of bodies. The solar system is a group of stars. When the solar system is considered as a unity the particles of which it is composed are the stars, but when one of these is studied as a unity it is found to be composed of particles. When any one of these particles is considered by itself it is a body. A molecule is a body considered as a molecule, but it is composed of many atoms, which are its particles. If, on the other hand, the atoms are compound, then they are bodies. Thus it is that a body is composed of particles, and that which is a body or system in relation to its component particles may be a particle in relation to a body or system of a higher order. It is in this sense that the term must be understood when we affirm that acceleration in a body or system is deflection of its particles. The ball in the hand is not at rest, or without motion in its particles; the horse has not more motion in its particles when running than when standing; the ship at anchor has motion still in its particles. These propositions are all

simple and can be easily demonstrated, and yet the illusion remains. These seeming paradoxes are to be explained if we affirm that motion cannot be created or destroyed.

It has been demonstrated by science that motion is persistent—cannot be created or annihilated, and the demonstration has been accepted by a great body of scientific men. Antecedent to this demonstration Newton had propounded three laws of motion, one of which is that action and reaction are equal and in opposite directions. In this axiom the persistence of motion or the indestructibility of energy was implied, but at first its full significance was not understood, perhaps not even by Newton himself.

In 'The Principia' his first chapter is a series of definitions, the third of which is as follows:

"The *vis insita*, or innate force of matter, is a power or resisting by which every body, as much as in it lies, endeavors to persevere in its present state, whether it be of rest or of moving uniformly forward in a right line.

"This force is ever proportional to the body whose force it is, and differs nothing from the inactivity of the mass, but in our manner of conceiving it. A body, from the inactivity of matter, is not without difficulty put out of its state of rest or motion. Upon which account this *vis insita* may, by a most significant name, be called *vis inertiae*, or force of inactivity. But a body exerts this force only when another force impressed upon it endeavors to change its condition, and the exercise of this force may be considered both as resistance and impulse; it is resistance, in so far as the body for maintaining its present state withstands the force impressed; it is impulse, in so far as the body, by not easily giving way to the impressed force of another, endeavors to change the state of that other. Resistance is usually ascribed to bodies at rest, and impulse to those in motion; but motion and rest as commonly conceived

are only relatively distinguished, *nor are those bodies always truly at rest which commonly are taken to be so.*"

In the last sentence quoted it is apparent that Newton himself was conscious of an illusion in the common conception of the term rest, and it is plain from his entire discussion that his term inertia stood for real force, although many scholars since his time have denied this proposition. Had Newton discovered the real nature of what he called *vis inertiae* 'The Principia' would have been simplified, as it has been since his time, by definitions given to momentum, energy, force and power. But even these newer definitions can be revised and the subject presented in a simpler manner. The purpose in view in this chapter is to re-define *vis inertiae*, and to explain the phenomenon of rest in molar bodies by showing that it is not annihilation of motion, but change in the direction of motion, and that the ordinary concept of rest in molar bodies is an illusion, and that this illusion has been carried into the realms of molecular and stellar motions.

Vis inertiae or inertia is a component of real force, inherent in every particle of matter as speed of motion, which can be changed in direction only through the agency of collision. The explanation of Newton's third law of motion in this manner changes the ideas of motion as they have heretofore existed in philosophy. Motion as speed is inherent, and not something imposed from without. If indeed, this be true, then much reasoning in scientific circles must be revised, for it has far-reaching results.

The correlation of forces through the persistence of motion or the persistence of energy is not universally accepted, but is widely accepted, and it seems to be growing in favor by reason of its great simplicity, and because it furnishes an explanation of many facts and a conceivable explanation for many more, but chiefly from the all-im-

portant consideration, attested again and again by observation, that motion is a real cause or antecedent of force and that no other cause is known. A second explanation of force is never even propounded except as a reification of abstractions inherited from the age of metaphysics, and still found as an atavism in science.

In the consideration of motion it is necessary to consider the two elements, namely, speed and direction, or path, for each term posits the other. The persistence of motion inheres in the element of speed. While the body in motion must have a path its direction is variable, *i. e.*, not persistent as a right line. It must therefore be understood that in speaking of the persistence of motion it is the element of speed to which reference is made. To affirm that motion is persistent is equivalent to the affirmation that speed is persistent, though the path of motion may change. It is not proposed here to discuss the conservation of energy nor the kinetic hypothesis that force is the collision of matter in motion, but to assume these theories for the purpose of exhibiting their logical consequences.

In every collision of one particle or body with another there is a double correlative involved. When A and B collide, A acts on B and B on A, so that there is both action and passion in A and B which are co-existent. Then we have to consider A before the collision and A after the collision, and B before the collision and B after the collision. There is thus a double cause and a double effect which are sequent. The matter may be expressed in another way. A and B coöperate in producing effects on each other. In this coöperation action and reaction are involved. The action is the cause and the reaction is the effect. How is the cause quantitatively related to the effect and how is the effect divided between them? It is proposed to prove that collision does

not produce any change in the speed of A or B, but the result of the collision is the deflection of the paths of both and this deflection is proportional to their masses. All this is simple in the collision of two free bodies of a certain class, both of which are in motion and which collide when their paths impinge upon each other. But two bodies, A and B impinge. A is without molar motion; B has molar motion. Will B yield a part of its motion to A, or will B retain its motion as in the case of two free moving bodies and create motion in A? Or if A is unmoved and B is stopped in its molar motion will motion be annihilated? If two molar bodies are free and both in motion and their paths impinge, neither particle has its speed increased or diminished, but if one is at rest it will be put in molar motion and it will thus appear to have motion given to it either by the creation of motion or by taking it from the other. The illusion involved arises from this, that the molar body said to be at rest is really not at rest. If they are both free and in motion it is plain that one does not yield motion to the other. But if one of the bodies is in the state called rest it appears that it is set in motion or that the other body is brought to rest. In the first case it seems that motion is not created nor annihilated, in the second that motion is created and in the third that motion is annihilated. Is this true? This is the question we are to answer. Can motion in any body be created or destroyed by collision? It appears so, but we are to show that this appearance is an illusion.

Every particle of matter known to man is in motion at a high velocity. This wooden ball is in motion about the axis of the earth, about the sun, and also with the sun about some other point in the heavens. The sum of all these motions considered as speed is unknown, but it may be affirmed with safety that it is very great. Let us call this the

telluric motion of the ball, its motion with the earth. Its path is composed of at least three contemporaneous revolutions. However great the speed of the telluric motion, it is yet small as compared with other motions within the body itself. As now understood the woody tissue is composed of cells, the cells of molecules, and the molecules of atoms, all grouped in such a manner by composed motion as to constitute a tissue whose structure is preserved by molecular motion. That rigidity is sometimes due to motion is well known. Stand by the nozzle of a monitor with four hundred feet of pressure behind the water and watch the stream drive the great boulder away. Strike this stream with a crowbar; though the iron may bend, the stream is unbroken. So we may conceive that rigidity and strength of structure are properties of motion. Let us call this rigidity and structural strength of the woody tissue constitutional motion, whose force is equal to the sum necessary to rend the ball into its constituent atoms. The structural strength is a measure of its constitutional motion, which is great as compared with any molar motion observed in the ball. Again the body exhibits a mode of motion known as heat, which is undulatory or vibratory. Of the speed of radiant heat something is known, and it is well-known that it is very great as compared with any molar motion observed in the bodies which exhibit the heat. Let us call this constitutional and thermic motion molecular motion.

I roll the ball over the floor, and molar motion is exhibited to the vision.

Thus we know of three kinds of motion possessed by the body, but that which is apparent to the unaided vision as molar motion is but a minute part of the whole. It is evident that it is a very small part of the telluric motion. Let us now see what proportion it bears to the molecular or the constitutional and thermic motions com-

bined. The constitutional motion is measured by the force with which the atoms, molecules and cells are held together as an organic body. If we attempt to realize this we find it very great, yet we cannot attain to its measure, from the fact that it is complicated with the heat motion of the body, but we can obtain some realization of the sum of the two kinds of motion, though we cannot with certainty divide the molecular motion between them.

Let us first consider the velocity of reasonably well-known molecular motion:

VELOCITY OF GAS MOLECULES.

	Meters per second.
Atmospheric air.....	485
Oxygen	425 to 458
Nitrogen.....	453 to 491
Hydrogen.....	1838 to 1841
Ammonia	628 to 737
Aqueous vapor.....	614

VELOCITY OF THE TRANSMISSION OF SOUND.

	Meters per second.
In air.....	333
“ oxygen	317
“ hydrogen	1270
“ ammonia	415
“ water	1435

But all of these same molecules have the motion of the earth, first about its axis, which at the equator is 465 meters per second, and in orbit 29,606 meters per second. Neglecting the motion of the earth with the sun about some other point in the heavens we still see that the known molecular motion, plus the known telluric motion, which we have considered, far exceeds any molar motion observed in nature or produced in art. The molecular motion of a cannon ball at its mouth is from 518 to 671 meters per second. In telluric motion we have the motion of bodies, and again in molecular motion we have the motion of bodies. The molecules themselves are compound, and in order that the molecular bodies themselves should retain their constitution it is necessary that the motion of

their particles should be made immensely composite as correlative motions. What idea can we obtain of the velocity of this particle motion? Take the wooden ball which we have considered and burn it and we have motion as light, and light is transmitted at the rate of 299,878,000 meters per second. Here we have particle motion at a velocity so great that any observed molecular motion sinks into insignificance; all of the ethereal motions seem to be at least of rudely commensurate magnitude. If the atoms are compound, as seems to be indicated by a large body of evidence obtained through chemical research, possibly it may be that the particles of atoms are commensurate with the particles of ether and that they have the same speed; but this hypothesis is not necessary to the present argument. It is only necessary to show that the molecular and constitutional motions, together with the telluric motions of every particle, are of such a magnitude as to fall far within the speed of molar motion.

None of these motions are persistently right-line motions. It is manifest that the stellar motions are great revolutions. The constitutional motions are also enormously composite. The heat motions, though they may be right-line motions in minute parts, must be composite motions, their paths forever changing, else the body would be dissipated. The particle motion of each particle in the molecule has its path confined to the sphere of the molecule itself. Considering this motion, both structural and thermic, not in relation to telluric motion nor in relation to molar motion, but wholly in relation to the particles of the molecule, it must be highly composite. The molar motion of the rolling ball is revolution and translation, but it is so small as compared with the others that it hardly seems worthy of consideration. Still it must not be neglected, for this is the motion the characteristics of which we have set out

to explain. Let us once more consider what has been said. The atoms of the ball, when all their motions are analyzed and summed, prove to have enormous velocities in enormously composite paths compared with which the molar motion of the ball on the floor sinks into insignificance.

Every particle in the wooden ball rolling on the floor has telluric motion, molecular motion and molar motion. Consider one of these particles moving with the three kinds of motion, and we realize that its speed is very great and that the path which it traverses is greatly composite. If such a particle had its composite path straightened into a right-line path it would at once pass out of the sphere of the solar system into a region beyond, from whatever point within the system it might start, and in whatever direction the right-line path extended. But the molecule remains within the solar system because its stellar motion is composite; and it remains within the ball because its molar motion is composite; and it remains within the molecule because its molecular motion is complete.

When the ball was started molar motion began and when it stopped that molar motion ended. But we do not suppose that it came out for nothing and vanished into nothing; we resort to preëxisting molecular motion to explain it; we say that the molar motion was derived from the molecular motion of the hand that set the ball rolling and that it was transformed into molecular motion in the wall which destroyed the molar motion. In making this explanation we assume that motion as speed went out of the hand into the ball and then out of the ball into the wall. Is this true? Was the velocity of the molecular motion in the hand diminished and the velocity of the molecular motion in the wall increased? If so, action and reaction are not equal except in the sense that what is lost by one is gained by the other.

Did motion go out of the hand into the ball, or was the direction of motion existing in the ball changed? Did motion go out of the ball into the wall, or was the direction of motion existing in the wall changed? If the law of action and reaction is valid, when the change was made upon the ball by the hand, an equal change was made upon the hand by the ball. Neither of them lost velocity by the changed form, or one lost what the other gained. All of Newton's reasoning on this subject proceeds upon the assumption that the speed of each is unchanged, but that the direction of each is changed and that this deflection is equal in the case now considered. When the ball struck the wall neither ball nor wall lost motion, but the molecular paths were changed by collision. The form or mode of direction of motion was affected, the quantity of motion as speed was unaffected, if we follow Newton's reasoning. But there was a change in the hand, in the ball, and in the wall. In what did that change consist? We know that in part at least it consisted in a change of paths. The molecular motions in the hand must have had their directions changed; the molecular motions in the ball must have had their directions changed; in like manner the molecular motions of the wall were changed in direction. This we know; in every collision there is a change of direction in the motion of the particles constituting the bodies colliding. Is this change of direction all? Or is there a transference of speed so that one loses while another gains? The whole problem is narrowed to this issue—that which we call acceleration is wholly deflection or in part deflection and in part loss and gain—loss of speed by one and gain by another, and if there is any loss and gain then action and reaction are not equal, as Newton's law affirms.

There is still another set of relations which must be considered. A body is con-

stituted of particles; that the motion of the particles within the body should remain within the sphere of the body, their paths must be composite. In order that their paths may be composite there must be a sufficient number of collisions to deflect these several particles and retain them within that sphere.

If the body itself is moved the paths of the several particles in the average must thus be rendered less composite, that is, the number of collisions must be diminished. The motion of the body as such, therefore, is accomplished by diminishing the deflections within the body and straightening their paths. The translatory motion of a body is a straightening of the paths of the particles of which the body is composed.

Imagine a man walking in a circle of ten feet radius. The sphere of his motion is within the circumference. He may soon walk a mile and never be more than twenty feet away from any given point in the circumference; change his direction so that his path is straightened, and he may soon be a mile away. A body of men walking in a circle remain together as a body within the circumference of the circle as it moves with the earth; change their paths to a cycloid and the body of men will move away or change their paths to parallel right lines, and as a body they may soon be a mile away and still in a circle. In the same manner the molecules of the wooden ball are in motion within the theater of the ball, so that they do not pass beyond its boundaries, yet impose upon each molecule a change of direction in such manner that they all move a little more in one course and a translation of the ball is affected by a change of direction in the motion of its constituent molecules, and the ball still remains as an incorporated body. It is thus possible to explain molar motion of the ball as a change in direction of the motion of its molecular parts, without assuming an increase of speed in

the parts. By such an assumption the molar motion perceived by vision would be legitimately derived from the molecular motion known by reason, and appear as a change of direction in the telluric motion of the ball.

No motion would be created or destroyed, and action and reaction would remain equal, while the apparent molar motion would be explained by a change of direction in molecular motions, very minute as compared with the composite paths of the several molecules and the composite path of the body in its telluric motion. When we consider the total motions of the atoms of the ball, even when it is shot from a cannon's mouth, an inconceivably small change of direction in the motion of every atom as compared with the complexity of its path would fully account for the flight of the ball as projected by dynamite.

Now we know of deflection and that it arises from collision, and we know of no other change in motion. Acceleration as increase of speed cannot in the nature of the case be demonstrated, for it may always be explained as deflection, and can never be explained without deflection; and to assume acceleration as increase of velocity is to contradict the law that action and reaction are equal and to affirm that motion can be created and destroyed.

If acceleration is explained as deflection, it is explained by referring it to a known cause and adequately explained.

Let this argument be stated in brief:

First, the tendency of modern investigation is to explain all forces as derived from modes of motion. Great progress has been made in this direction, and the theory is widely accepted.

Second, all understood forces are collisions.

Third, if all forces are collisions the motions from which they result obey the third law of motion, that action and reaction are equal. By this law it is seen that no mo-

tion as speed can be lost or gained by any particle of matter.

Fourth, by collision paths can be changed, but motion as speed cannot be transmitted.

Fifth, in molar motion there is an apparent creation and annihilation of motion, but this appearance is known to be an illusion. It has been explained as due in part to collision and in part to the transmission of motion. Acceleration, therefore, must be something else than an increase of speed. It is known to be in part deflection and can all be thus explained; and if the first law of motion is valid it is thus explained. Therefore:

1. Molar acceleration is deflection of molecules.
2. Speed of motion in matter is constant.
3. The direction of motion is variable.
4. Speed is inherent in matter and is not imposed upon it from without.
5. The path of motion is controlled by environment.

The laws of motion propounded by Newton can be more simply stated as follows:

Law I. The velocity of motion is persistent.

Law II. By the collision of two bodies the direction of their motions is changed in equal components.

Vis inertia is the power which particles have of deflecting each other by collision, due to their persistent motion.

Every particle has perpetual motion as speed which can not be increased or diminished, and the absurdity of perpetual motion should be called the absurdity of perpetual collision. The particles collide because of impinging paths; they are deflected and their paths are turned apart and they cannot be made to collide again until other external collisions bring their paths together. If the particle A after one collision is once more deflected, another collision is necessary. It is thus that the absurdity of perpetual collision can be simply

demonstrated. After such an analysis the explanation of gravity as the mutual protection from impinging particles becomes simple, the doctrine of virtual velocities self evident; and there are many other consequences of this law which, properly understood, would make many propositions of physics self-evident.

It must be clearly understood that the above argument does not deny that the motion of a body cannot be accelerated in speed; such a denial would be an absurdity. Every particle of which we have knowledge is a constituent of many bodies in a hierarchy of bodies and what is here affirmed is that the acceleration of a body in speed is deflection of its particles, and that embodiment itself is always a result of deflection in the particles embodied. A molar body may have its molar motion increased or diminished in speed by deflecting its molecular motions. If the speed of a molar body be changed, the direction of its molecular particles must necessarily be changed. This proposition is self-evident. The third law of motion is equally simple. The law here demonstrated affirms that acceleration in one embodiment is deflection in another and it makes valid Newton's law, which would be an absurdity were the law here demonstrated untrue; and if untrue the persistence of motion is an absurdity, and with it the persistence of energy falls to the ground.

J. W. POWELL.

SCIENTIFIC NOTES AND NEWS.

ASTRONOMY.

THE *Astronomische Nachrichten* of February 22d contains an article by Dr. H. F. Zwiers, giving a new method of computing double-star orbits, and an application of it to the orbit of Sirius. The author does not claim great precision for his orbit of his star, and it is given simply as an illustration of his method of computation. We do not think, however, that the method will commend itself very greatly to astronomers. Glasenapp has pointed out (*Orbites*

des étoiles doubles, p. q.), that the application of graphical methods to the problem in question ought to cease with the drawing of the apparent ellipse. After this has once been drawn, the computation by Kowalski's elegant formulæ does not require more than half an hour.

THE same journal contains an account of some very interesting experiments which have been made at the Munich Observatory, by Dr. Schwartzschild. A new form of micrometer has been constructed, using the principle first employed in 1891 by Michelson for the measurement of the satellites of Jupiter. This new micrometer has been applied with the help of a ten-inch telescope to the measurement of a number of close double stars. Briefly stated, the new instrument consists of a movable plate, pierced with several slits, and mounted outside the object glass of the telescope. This produces a series of spectra of both the principal star and the companion in the field of view of the telescope. By revolving the slit plate until the spectra of both stars are all in a straight line in the field of view, it is possible to measure the position angle. Similarly, by a sliding motion of the slit plate, the spectrum of the companion can be made to appear exactly midway between two neighboring spectra of the principal star. From a reading of the scale attached to the slit plate it is then possible to compute the angular distance of the component from the principal star. The whole apparatus is very simple and inexpensive, and could be applied easily to any equatorial provided with a position micrometer. Thirteen stars have been measured with this instrument by two observers. The distance for the closest double is $0''.86$, while the greatest distance measured was $4''.25$. Distances over five seconds could not be measured accurately, because at this distance the spectra begin to show too much color for accurate observation. The probable errors of these observations compare very favorably, indeed, with those obtained for other forms of micrometric apparatus, especially in the case of very small distances. But as is well known, the very close doubles are the ones most important to measure.

THE last number of the *Astrophysical Journal*

contains an account of the progress made with the new observatory of the University of Chicago. An interesting feature of the new institution is to be a complete optical and mechanical instrument maker's outfit. And an optician as well as an instrument maker are to be permanently attached to the observatory staff.

H. J.

MARINE ORGANISMS.

THE Friday evening discourse at the Royal Institution on February 29th was delivered by Dr. John Murray, of the Challenger expedition, who spoke on 'Marine Organisms and their Conditions of Environment.' According to the report in the London *Times* Dr. Murray pointed out that in the distribution of marine organisms temperature was a more important factor than in the case of air-breathing and warm-blooded animals on the land surfaces, although in the ocean the extreme range of temperature never exceeded 52 deg. Fahr. In the surface waters of the ocean there were five well-marked temperature areas—an Arctic and an Antarctic circumpolar belt with a small range and a low temperature, a circumtropical belt with a small range but a high temperature, and two intermediate areas with large annual ranges of temperature. The waters of the ocean might be divided into two great regions—the superficial region down to about 100 fathoms, and the deep-sea region. In the former, and especially in the marginal zone surrounding the land, there was great variety of conditions and an abundant fauna and flora, whereas under the uniform conditions found in the deep-sea plant life was absent, though there was animal life in abundance. In the warm surface waters of the tropics there were many species, but relatively few individuals, while the reverse condition was found in polar areas. Again, in tropical pelagic regions organisms secreting carbonate of lime were abundant, but gradually disappeared towards the poles. In the warm waters the pelagic larvæ of bottom-living species were always found, but in the cold appeared to be absent. The lecturer was of opinion that the various facts in the distribution of marine organisms might be accounted for by supposing that in early geological times

there was a uniform climate over the whole surface of the globe and an almost universal fauna and flora. The coral reefs that flourished within the Arctic Circle in the Palæozoic period were formed when the water in the polar regions had probably a temperature approaching 70 deg. Fahr., and when cooling set in those animals with pelagic larvæ and those which secreted carbonate of lime would either succumb or be forced to retire to warmer waters, those having a direct development surviving. Cold water descending from the poles into the deep sea would carry oxygen with it and render the deep regions habitable, thus initiating migrations from the mud line. The elimination of the same elements, in the manner indicated, from the two polar faunas would account for their resemblance and even identity, as well as for the similarity of the polar and deep-sea faunas and the absence of truly ancient types in the deep sea.

GENERAL.

It is expected that there will be present an unusual number of foreign guests at the Liverpool meeting of the British Association (Sept. 16-23). A special scientific excursion to the Isle of Man has been provided. The geology of this island is varied and interesting, especially as regards igneous and glacial formations, and fossil-bearing carboniferous limestones; the Prehistoric, Scandinavian and other early remains are celebrated, the marine fauna and flora are abundant, and the presence of the Liverpool Marine Biological Station at Port Erin will be a special attraction to all naturalists. Prof. W. A. Herdman, of University College, Liverpool, is chairman of the local committee.

THE Polarizing Photochronograph devised by Lieut. G. O. Squier, U. S. A., and Mr. Albert C. Crehore, has been recognized by the Franklin Institute of Philadelphia, which has given them the John Scott endowment medal for 1895.

THE life of Prof. A. W. von Hoffman, founder and long president of the German Chemical Society, will be written by a committee of the Society, composed of his successor, Prof. E. Fischer, Dr. Martius and Prof. F. Tremann.

THE scholarships for some time maintained at the Naples Zoölogical Station by the Universities of Oxford and Cambridge are to be continued.

PROF. SIMON NEWCOMB has been elected a member of the Royal Academy of the Lincei at Rome, and also made an officer of the French Legion of Honor.

PROF. H. A. ROWLAND, of Johns Hopkins University, has been made an officer of the French Legion of Honor, foreign correspondent of the French Academy of Sciences, and foreign member of the Italian Society of Spectroscopists.

ALL teachers of natural science are invited to join in a movement to raise the requirements in science for admission to college, by attending the next meeting of the National Educational Association at Buffalo, July 3-11, 1896. At the Denver meeting, 1895, a Department of Natural Science Teaching was organized, as a regular part of the National Educational Association, with the following officers: Prof. C. E. Bessey (Lincoln, Neb.), President; Prof. Wilbur S. Jackman (Cook County Normal School), Vice-President; Prof. Chas. S. Palmer (Boulder, Colo.), Secretary. The Western States have taken the lead, but it is hoped that all college and high-school teachers of science will unite in the movement. A good program, including special papers on the various topics in physics, chemistry and biology, is now being arranged and will soon be published.

PROF. M. I. PUPIN, of Columbia University, will lecture in the New York Academy of Sciences on March 23d, on 'Röntgen's Discovery.' The lecture will be illustrated by experiments and lantern views.

MACMILLAN & Co. have issued cards of the standard library size giving the publications of the Columbia University press. In addition to the ordinary bibliographical details each card contains a synopsis of the contents of the volume. The cards need to have only the library reference number added and can then be placed without further copying in the card catalogue.

A BRONZE memorial tablet in memory of the late Prof. George Huntingdon Williams, who

occupied the chair of inorganic geology at the Johns Hopkins University, will be placed in the Williams memorial room of the geological laboratory, which contains the collections made by Prof. Williams.

Garden and Forest states that the Puget Sound University owns what is called a residence park of some twelve hundred acres southwest of the city of Tacoma, and it is proposed to devote some two hundred acres of this, where the soil is most suitable, to an arboretum of such trees as will grow in the remarkable climate of that region. The amount of land available is so ample that room can be given for a large collection. Some ten thousand young plants of two hundred and fifty species, native and foreign, already form the nucleus of the proposed tree museum.

THE first two papers of Vol. VIII., of the Bulletin of the American Museum of Natural History are by Mr. Frank M. Chapman, and discuss the changes of plumage in the Dunlin and Sanderling and in the Snowflake. Herr Gätke says that the change of color in the Dunlin and Sanderling takes place without molt and is due to changes in the feathers themselves, but Mr. Chapman shows that in passing from winter to summer plumage the Dunlin undergoes a complete molt of the body feathers and scapulars, but retains the rectrices and remiges; the change in the Sanderling is also due to molting. In regard to the Snowflake, Mr. Chapman states that they molt once a year, after the breeding season, and that the difference between the dress of September and that of the following spring is due to a wearing away of the edges of the feathers by which both their shape and color are changed.

M. DEPERRET, professor at Lyons, has described before the Paris Academy remains of dinosaurs found in Madagascar twenty-five miles south of Majunga. These seem to show close affinities with the fossils of British India.

As we have already had occasion to state, the work of a large proportion of the physicists of the world seems to have contributed but little to the results published by Prof. Röntgen, though an exception should be made in the case of the paper presented to the Royal Society by

Prof. J. J. Thomson. It is perhaps not surprising that the daily newspapers should publish all sorts of reports, even seriously explaining how at the College of Physicians and Surgeons, New York, the Röntgen rays are used to reflect anatomical diagrams directly into the brains of students, making, we are informed, much more enduring impressions than the ordinary methods of learning anatomical details. It seems, however, unfortunate that *Nature* should publish from its 'American correspondent' unconfirmed newspaper reports and that the Paris Academy should admit five consecutive papers on 'dark light,' apparently without scientific validity.

It is stated in *Electricity* that, in connection with the Electrical Exposition to be held in New York during May, arrangements have been made for an interesting historical and loan exhibit, to which it is intended to devote considerable space on the main floor. A committee composed of T. Comerford Martin, Dr. Park Benjamin and E. L. Morse has charge of the exhibit. Dr. Benjamin has one of the finest libraries in the world of early books on electricity, and these will be shown in cases arranged chronologically, with explanatory notes, portraits, autographs, etc. Mr. Morse, the son of Prof. S. F. B. Morse, is the possessor of an invaluable collection of telegraphic relics, curios, documents, etc., including his father's note books and sketches, all of which will be shown. Mr. Martin, besides owning many objects of interest connected with the early days of electricity, has secured from Mr. Tesla, Prof. Elihu Thomson, Mr. Edison, Mr. Edward Weston, Mr. Stieringer and others the loan of early and interesting apparatus.

At the anniversary meeting of the Geological Society of London, on February 22d, the officers for the ensuing year were elected as follows: President, Henry Hicks; Vice-Presidents, Prof. T. G. Bonney, Prof. A. H. Green, R. Lydekker and Lieutenant-General C. A. M'Mahon; Secretaries, J. E. Marr and J. J. H. Teall; Foreign Secretary, Sir John Evans; Treasurer, W. T. Blanford. The Council were also appointed. The retiring President, Dr. Henry Woodward, delivered his anniversary address, which dealt

with the life history of the crustacea in later palæozoic and in neozoic times. The Wollaston medal was awarded to Prof. E. Suess, the Murchison medal and part of the proceeds of the Murchison fund to T. Mellard Reade, and the Lyell medal and part of the proceeds of the Lyell fund to A. Smith Woodward.

THE Academy of Science of the University of Oregon was organized at Eugene, Ore., on January 10th. A constitution was adopted and Prof. Condon was elected President; Dr. T. W. Harris, Vice-President, and Prof. F. L. Washburn, Secretary and Treasurer. At the first regular meeting, which was held on January 25th, Prof. Condon read a paper on 'Two recently discovered fossils,' and several informal communications were presented.

PROF. AGASSIZ and his party, which includes Dr. W. McM. Woodworth and Dr. A. G. Mayer, are now in San Francisco, and will sail shortly for Australia in the steamship *Monowai*. A steamer has been chartered in Australia for the expedition to the Great Barrier Reef.

MR. J. B. HATCHER, of Princeton College, special agent and collector for the Bureau of Ethnology at Washington, and Mr. O. A. Peterson, collector for the American Museum of Natural History, New York, have embarked for Patagonia on the steamship *Galileo*.

A PARISIAN company has placed pneumatic tires on twenty of its cabs. It is claimed that these not only add greatly to the comfort of those using them, but also effect an actual economy. The average cost for repairs on a Paris cab is about 50 cents a day, and it is said that the pneumatic tires reduce this to one-half. The weight saved in the tires is about 100 lbs. and the whole vehicle may be built more lightly. It is also probable that even apart from the decrease in weight it is easier for a horse to draw a carriage with pneumatic tires.

WE take from *Natural Science* the following items: An expedition of sixteen men, headed by Dr. Cook, has started in two small vessels of 100 tons each for the bay of Erebus and Terror. Six of the men are students of science. The naturalists, Messrs. Austen and Cambridge, on the Siemens telegraph expedition to the Amazon, have already begun successful operations,

the fact that the 'Faraday' was stuck for a whole week on a mud-bank at the west end of Parana de Buyassu in no wise interfering with the aims of the collectors. The chief find at present has been two specimens of *Peripatus*, belonging apparently to different species. The naturalists decided to stay at Santarem, while the 'Faraday' proceeded to Manaus, which place it reached on February 8th, all well. Prof. H. de Lacaze Duthiers will, as in former years, conduct an excursion at Banyuls during the Easter vacation, that is, from March 28th to April 11th. Those joining the party can obtain return tickets from Paris to Banyuls for 46 francs. Among those who will attend are Professors Von Graff, of Graz; Pruvot, of Grenoble, and Yung, of Geneva, and probably some naturalists from Barcelona. The Professor desires to extend through us a cordial invitation to any English naturalists. The hydrographical exploration of the Skagerack has just been begun under the auspices of the Swedish government and the direction of Prof. O. Pettersson.

UNIVERSITY AND EDUCATIONAL NEWS.

THE bill establishing a National University of the United States has been reported favorably by the Senate committee. It grants a charter to the University, provides for its government, grants it the ground in the city of Washington designated by President Washington as a site for a national university, and appropriates \$15,000 for the fiscal year ending on June 30, 1897, and \$25,000 for the year following.

At the recent meeting of the Board of Trustees of the College of New Jersey at Princeton it was voted to change the charter name of the institution to Princeton University. The fund which is being raised in commemoration of the Sesquicentennial next October is already over \$900,000, a large proportion of which, it is said, will be devoted to the development of the graduate department.

At a special meeting of the Yale corporation it has been decided to construct a new dormitory on York street to cost \$100,000.

PROF. JAMES SETH, now of Brown Univer-

sity, has been elected professor of ethics in Cornell University.

THE promotion of Associate Prof. George F. Atkinson to the professorship of botany at Cornell University will be followed by a reorganization of the courses of instruction in the department which will go into effect at the opening of the coming year. Assistant Prof. W. W. Rowlee has been promoted to the highest grade of assistant professor; E. J. Durand, Sc. D., has been appointed instructor in botany, and K. M. Wiegand, assistant. The following advanced and graduate courses in botany are offered for the coming year: By Prof. Atkinson and Instructor Durand, comparative morphology and embryology, mycology and algology. By Assistant Prof. Rowlee and Assistant Wiegand, comparative histology, systematic botany and dendrology.

ADJUNCT PROF. W. H. ECHOLS has been elected by the Board of the University of Virginia to the full chair of mathematics to succeed Prof. C. S. Venable, who retires on account of ill-health. J. Morris Page, of Johns Hopkins University, has been elected adjunct professor.

THE senate of Cambridge University has rejected the proposition to appoint a committee to consider the question of conferring degrees upon women by a vote of 186 to 171.

DISCUSSION AND CORRESPONDENCE.

HEREDITY AND INSTINCT.*

IN his able posthumous work on *Post-Darwinian Questions, Heredity and Utility*, the lamented G. J. Romanes sums up the evidence for the inheritance of acquired characters in the final statement that only two valid arguments remain on the affirmative side; and to each of these arguments he has devoted considerable space. One of these arguments is from what he calls 'selective value,' and the other from the 'co-adaptations' found in the instincts of animals. He says (p. 141): "Hence there remain only the arguments from selective value and co-adaptation." If we take the in-

stincts as illustrating also the application of the principle of 'selective value,' we may gather the evidence which Mr. Romanes was disposed to cling to for the inheritance of acquired characters into a single net, and enquire as to the need of resorting to the Lamarckian factor in accounting for the origin of instinct. I wish to suggest some considerations from the psychological side, which seems to me entirely competent to remove the force of these two arguments, and to show to that extent that the instincts can be accounted for without appeal to the hypothesis of 'lapsed intelligence,' as the use-hypothesis, as applied to this problem of instinct, is called; in other words, to show that Darwin and Romanes were not correct in considering instinct as 'inherited habit.'

The argument from co-adaptation requires the presence of some sort of intelligence in an animal species; the point being that since the coördination of muscular movements found in the instincts are so co-adapted they could not have arisen by gradual variations. Partial adaptations tending in the direction of an instinct would not have been useful; and intelligence alone would suffice to bring about the coördinations which are too complex to be accounted for as spontaneous variations. These intelligent coördinations then become habits by repetition in the individual and show themselves in later generations as inherited habits due to 'lapsed intelligence.' Assuming, then, with Romanes—whom we may take as the most recent upholder of the view—the existence of some intelligence in a species antecedently to the appearance of the instinct in question, we may be allowed that supposition and resource.

I. But now let us ask how the intelligence brings about coördinations of muscular movement. The psychologist is obliged to reply: Only by a process of selection (through pleasure, pain, experience, association, &c) from certain alternative complex movements which are already possible for the limb or member used. These possible combinations are already there, born with him, or resulting from his previous habits. The intelligence can never, by any possibility, create a new movement, or effect a new combination of movements, if the

* Discussion (revised), following Prof. C. Lloyd Morgan before the New York Academy of Sciences, January 31, 1896.

apparatus of brain, nerve and muscle has not been made ready for the combination which is effected. As far as there are modifications in the grouping, even these are very slight functional variations from the uses already made of the muscles involved. This point is no longer subject to dispute; for pathological cases show that unless some adequate idea of a former movement made by the same muscles, or by association some other idea which stands for it, can be brought up in mind the intelligence is helpless. Not only can it not make new movements; it can not even repeat old habitual movements. So we may say that intelligent adaptation does not create coördinations; it only makes functional use of coördinations which were alternatively present already in the creature's equipment.*

Interpreting this in terms of congenital variations, we may say that the variations which the intelligence uses are alternative possibilities of muscular movement. But these are exactly the variations which instinct uses, except that in instinct they are not alternative. That this is so, indeed, lies at the basis of the claim that instinct is inherited habit. The real difference in the variation involved in the two cases is in the connection in the brain whereby in instinct the muscular coördination is brought into play *directly* by a sense stimulation; while in intelligence it is brought into play *indirectly*, *i. e.*, through association of brain processes, but by the same sense stimulation or a similar one. Now this difference in the central brain connections is, I submit, not at all a great one relatively speaking, and it might well be due to spontaneous variations. The point of view which holds that great co-adaptations of *musculatur* are to be acquired *all at once* by the creature is quite mistaken.

The same class of considerations refutes the argument from 'selective value.' This argument holds that the instinct could not have arisen by variations alone, with natural selec-

tion, since partial coördination tending in the direction of the instinct would not have been useful; so the creatures with such partial coördinations merely would have been killed off, and the instinct could never have reached maturity; only variations which are of sufficient value or utility to be 'selective' would be kept alive and perfected.

But we see that the intelligence which is appealed to, to take the place of instinct and to give rise to it, uses just these partial variations which tend in the direction of the instinct; so the intelligence *supplements* such partial coördinations, makes them functional, and so *keeps the creature alive*. In the phrase of Prof. Lloyd Morgan, this prevents the 'incidence of natural selection.' So the supposition that intelligence is operative turns out to be just the supposition which makes the use-hypothesis unnecessary. Thus kept alive, the species has all the time necessary to perfect the variations required by a complete instinct. And when we bear in mind that the variation required is, as was shown above, not on the muscular side to any great extent, but in the central brain connections, and is a slight variation for functional purposes at the best, the hypothesis of use-inheritance becomes not only unnecessary, but to my mind quite superfluous.

II. There is also another great source open to the Neo-Darwinian in this matter of instinct; also a psychological resource. Weismann and others have shown that the influence of animal intercourse, seen in maternal instruction, imitation, gregarious coöperation, etc., is very important. Wallace dwells upon the actual facts which illustrate the 'imitative factor,' as we may call it, in the personal development of young animals. I have recently argued that Spencer and others are in error in holding that social progress demands the use-hypothesis;* since the socially-acquired actions of a species, notably man, are socially handed down; giving a sort of 'social heredity' which supplements natural heredity. And when we come to enquire into the actual mechanism of imitation on the part of a young animal we find much the same sort of function involved

* When we strain our muscles to accomplish a new act of skill we are aiming to use the apparatus in new ways by a selection from possible combinations; and even when we learn to use disused muscles, as those of the ear, we are only stirring up old connections.

* SCIENCE, August 23, 1895, summarized in *Nature*, Vol. LII., 1895. p. 627.

as in intelligent adaptation. The instinct to imitate requires a general tendency to act out for himself the actions which the animal sees, to make the sounds which he hears, etc. Now this involves connections of the centers of sight, hearing, etc., with certain muscular coördinations. If he have not the coördinations he can not imitate; just as we saw above is the case with intelligence, if the creature have not the apparatus ready, he can not use it intelligently. Imitation differs from intelligence in being a general form of coördinated adaptation, while intelligence involves a series of special forms.* But both have to have the apparatus of coördinated movement. So we find, as an actual fact which all agree upon, that by imitation the little animal picks up directly the example, instruction, mode of life, etc., of his private family circle and of his species. This then enables him to use effectively, for the purposes of his life, the coördinations which become instincts later on in the life of the species; and again we have here two points which directly tend to neutralize the arguments of Romanes from 'selective value' and 'co-adaptation.' The co-adaptations may be held to be gradually acquired; since the coördinations of a partial kind are utilized by the imitative function before they become instinctive. And the law of 'selective value' does not get application, since the imitative function, by using these muscular coördinations, supplements them, secures adaptations, keeps the creature alive, prevents the 'incidence of natural selection,' and so gives the species all the time necessary to get the variations required for the full instinctive performance of the function.

III. These positions are illustrated in a very fortunate way by the interesting cases reported by Prof. Morgan in his discussion this evening. He cites the beautiful observation that his young chicks had the instinct to drink by throwing their heads up in the air, etc., but that it came into action only after they had the taste of water by accident or by imitating the old fowl. As Mr. Morgan says, the 'incidence of natural

selection' is prevented by the imitation or instruction or intelligent adaptation (in cases where experience is required). So, in this instance, the instinct of drinking, which has only got so far as a connection of certain muscular coördinations with the sense of taste, is made effective for the life interests of the chick. Thus kept alive the species has plenty of time—in case it should be necessary—to get a connection established also between the sight center and the same coördination of movements; so that future chicks may be born with a capacity for drinking when water is seen only without waiting for instruction, a fortunate accident, or an example to imitate. So we may imagine creatures, whose hands were used for holding only with the thumb and fingers on the same side of the object held, to have first discovered, under stress of circumstances and with variations which permitted the further adaptation, how to make intelligent use of the thumb for grasping opposite to the fingers, as we now do. Then, let us suppose that this proved of such utility that all the young that did not do it were killed off; the next generation following would be intelligent or imitative enough to do it also. They would use the same coördinations intelligently or imitatively to prevent natural selection getting its operation; and so instinctive 'thumb-grasping' might be waited for indefinitely by the species and then be got altogether apart from use-inheritance.

We may say, therefore, that there are two great kinds of influence, each in a sense hereditary; there is *natural heredity* by which variations are congenitally transmitted with original endowment, and there is '*social heredity*' by which functions socially acquired (*i. e.*, imitatively, covering all the conscious acquisitions made through intercourse with other animals) are also socially transmitted. The one is phylogenetic; the other ontogenetic. But these two lines of hereditary influence are not separate nor uninfluential on each other. Congenital variations, on the one hand, are kept alive and made effective by their conscious use for intelligent and imitative adaptations in the life of the individual; and, on the other hand, intelligent and imitative adaptation become congenital by further progress and refinement of

* That they are really the same in type and origin I have argued in detail in my work *Mental Development in the Child and the Race* (2d ed., Macmillans, 1895).

variation in the same lines of function as those which their acquisition by the individual called into play. But there is no need in either case to assume the Lamarckian factor.

The intelligence holds a remarkable place in each of these categories. It is itself, as we have seen, a congenital variation: but it is also the great agent of the individual's personal adaptation both to the physical and to the social environment.

The emphasis, however, of the first of these two lines of hereditary influence gives prominence to instinct in animal species, and that of the other to the intelligent and social coöperation which goes on to be human. The former represents a tendency to brain variation in the direction of fixed connections between certain sense centers and certain groups of coöordinated muscles. This tendency is embodied in the white matter and the lower brain centers. The other represents a tendency to variation in the direction of alternative possibilities of connection of the brain centers with the same or similar coöordinated muscular groups. This tendency is embodied in the cortex of the hemispheres. I have cited 'thumb-grasping' because we can see in the child the anticipation, by intelligence and imitation, of the use of the thumb for the adaptation which the simian probably gets entirely by instinct, and which I think an isolated and weak-minded child, say, would also come to do by instinct.

IV. Finally there are two general bearings of the position taken above regarding the developmental function of intelligence and imitation which may be briefly noted:

1. We reach a point of view which gives to organic evolution a sort of intelligent direction after all; for of all the variations tending in the direction of an instinct, but inadequate to its complete performance, *only those will be supplemented and kept alive which the intelligence ratifies and uses for the animal's personal adaptations.* The principle of selective value applies to the others or to some of them. So natural selection kills off the others; and the *future development of instinct must at each stage of a species' development be in the directions thus ratified by intelligence.* So also with imitation. Only those imitative actions of a creature

which are useful to him will survive in the species; for in so far as he imitates actions which are injurious he will aid natural selection in killing himself off. So intelligence, and the imitation which copies it, will set the direction of the development of the complex instincts even on the Neo-Darwinian theory; and in this sense we may say that consciousness is a 'factor' without resorting to the vague postulates of 'self-adaptation,' 'growth-force,' 'will-effort,' &c., which have become so common of late.

2. The same consideration may give the reason in part that instincts are so often coterminous with the limits of species. Similar structures find the similar uses for their intelligence, and they also find the same imitative actions to be to their advantage. So the interaction of these conscious factors with natural selection brings it about that the structural definition which represents species, and the functional definition which represents instinct, largely keep to the same lines.*

J. MARK BALDWIN.

PRINCETON UNIVERSITY, February 5, 1896.

INSTINCT.

EDITOR OF SCIENCE: Some remarks appended to my letter published in SCIENCE No 62, on the subject of Prof. Morgan's views on instinct by 'The Writer of the Note,' in view of the importance of the subject are worthy of further consideration.

Before drawing conclusions from observations on domestic animals, it is well to consider similar facts in connection with their wild congeners, especially if such conclusions are of a far-reaching character, and it cannot be too well borne in mind that our experiments are very clumsy imitations of nature in a large proportion of cases.

* In conversation with Prof. Lloyd Morgan I was glad to find that he was inclined to interpret the facts which I have quoted from him (and others) in somewhat the same way—that is, as pointing to general conclusions similar to those reached above. While I have reached my conclusions quite independently and from a psychological point of view, any confirmation which they get from so expert and eminent a biologist gives them much greater weight.

If food be set down in considerable quantity before newly hatched chicks, and in a vessel similar to that in which water is usually held, they will be relatively slow to recognize and eat such food. But in a wild state the congeners of the domestic fowl, as grouse, pheasants, etc., do not find food or water before them in such way. Their food is distributed, however, much more like the particles we scatter before the chick than does their water supply resemble that of our methods.

A young grouse would naturally get its water from the dew on herbage, possibly from rain water that had gathered in little hollows of the ground, surface, etc. And when the birds approach a stream the surface near is moist or wet, the particles it would naturally peck at would be found up to and beyond the very margin of the water, so that the contact of the beak with water in all these cases would be inevitable and drinking would come about as naturally as eating.

When the 'writer of the note' says, 'A chick swallows water instinctively, but must be taught to drink by example or accident,' the latter term evidently having reference to the observation specially described in my letter, he plainly either misses the real point of my observation or neatly evades it. One might as well say a puppy learns to smell by accident, for in the case in question the chick did not swallow water merely, but raised its head like an old fowl and *drank* perfectly well on the very first occasion that its beak had ever been immersed in water (as a puppy sucks when its lips first come in contact with a teat, etc.); and this I take it is what happens in nature. The young grouse in the forest, or even the chick on a grass plot or in a garden, would come in contact with water without any assistance from the mother bird.

The assumption that 'the chick might die of thirst in the presence of water, as the sight of water does not call up the movements of pecking at it as do food and other small objects,' is purely gratuitous. It is not primarily so much the sight, but rather the touch of water, inevitable, as I have tried to show, in a wild state that in the very first instance leads to drinking, though the bird would also peck at shining dew drops, as my chick did at the drops on the rim of a

vessel containing water. With a fair chance and plenty of water about in a condition at all resembling that in nature, there is no such thing for a vigorous, hardy chick as death from thirst.

That habits may be hereditary in dogs I have many times observed in my own kennel during the last eight years, and, without expressing any opinion as to the origin of instincts now, I can see no impossibility in their dating back to habits.

A doctrine which asserts that eating is instinctive, but that drinking is not, is to my mind one to marvel at, and is a poor foundation for theories of evolution or heredity.

Comparative psychology will, I fear, continue to suffer till those who assume to deal with it authoritatively spend more time among animals, and less in their studies. A few observations or experiments do not give them insight into the psychic nature of animals, and it were well, I venture to think, if the qualifications of the comparative psychologist, as set forth by Dr. Groos, in the preface to his admirable work, "*Die Spiele der Thiere*," were thoroughly known and believed in by all psychologists.

WESLEY MILLS.

MCGILL UNIVERSITY, MONTREAL.

PECULIAR ABRASION OF TREE TRUNKS.

PASSING recently through a tract of rather open forest land, I could not help but notice a very peculiar appearance or color showing to a nearly uniform height on the westward side only of many trees of different species.

This shade of dull yellow extended from the surface of the snow to a height of about three to four feet, and at a little distance had much the appearance of a fungoid growth which often may be seen in nearly this color on dead or decaying trees.

At first I was completely deceived, thinking it to be a growth of this nature, and wondering why it should have attacked so many trees at the same time, I proceeded to investigate. A close examination at once revealed the truth of the matter. It was a plain case of wind-carried snow and sleet versus tree trunks, and the outer moss-grown bark had succumbed as its cut and abraded surface made plain. In places this

abrasion amounted to almost a polish, at once bringing to mind the published descriptions of the cutting, polishing and sometimes complete destruction of tree trunks in portions of the southwest by flying sand.

To clearly show the entire possibility of the abrasion in this case being due to flying snow or sleet, I would state that the woodland wherein the phenomena was noticed is very open, of scattering growth and constitutes the northwestern border of a forest of small extent, having an open exposure to the westward of upwards of a mile. Thus the prevailing westerly winds, which rage with tremendous severity at times through this open tract, are able during the winter to hurl and sift through this thin forest growth tons of snow and icy sleet. This is evidenced by the enormous snow-banks which yearly form in the forest, at a little distance from its margins, in short, at the point where the wind by meeting repeated resistance loses its carrying power. This line of deposit varies, governed by the surface contour and variable density of forest growth.

Possibly the phenomenon described has been noticed and published before, but having access to considerable literature on forestry, I have never as yet met with any account, hence this slight contribution which may be of interest to some of the readers of SCIENCE.

PERCY M. VAN EPPS.

GLENVILLE, N. Y.

THE PUMA, OR MOUNTAIN LION.

DURING last July and August I was encamped with my family up on the Strait of San Juan de Fuca, near Port Williams, Clallam county, about thirty miles west of Port Townsend. One afternoon, while my children, with their nurse, were playing upon the beach in front of our cabin, a mountain lion (*Felis concolor* Linn.) came down through a strip of woods to the low bank overlooking the beach, and gave utterance to a most frightful cry or scream. I hastened out, calling loudly, and the commotion made by myself, wife, children and nurse, frightened away the brute. Although I had a Winchester repeating rifle in the cabin, I was unable to attempt to get a shot, by reason of a severe illness with which I had been prostrated for several weeks. I

heard this wild cry repeated several times afterwards, but each time farther away in the forest.

About two years before a Mr. Travis, a rancher, living near our camp, was returning home after dark, on horseback, and was chased by a lion. The horse fled in terror along the trail through the forest, never stopping until reaching home. Mr. Travis thinks that the attack was incited by a small dog that accompanied him, rather than upon himself or his horse. He returned the next morning to the locality with several hunting dogs and succeeded in shooting the animal, which proved to be a very large specimen, measuring eight feet from tip to tip. The lions are comparatively plentiful in all wild and thinly settled portions of the State.

I have written this sketch at the suggestion of Mr. Frederick W. True, of the Smithsonian Institution, author of an interesting illustrated paper on 'The Puma or American Lion,' published under the auspices of the Institution in 1891. In this paper Mr. True refers to a conflict of authorities in regard to the cries or screams of the animal, and also in regard to its belligerency, or rather, possibly, its timidity.

MERIDEN S. HILL,

Corresponding Secretary, Tacoma Academy of Science.

TACOMA, WASHINGTON, February 13, 1896.

LOGIC AND THE RETINAL IMAGE.

WHILE admitting that *all* the physiological antecedents to the sensation of vision are entirely outside the bounds of our experience in the use of eyes, your correspondent, C. L. F. (SCIENCE, February 7, 1896, p. 201), and many others who have written to this journal on the subject during the last six months, object to my assertion that I find *one* of these phenomena inconceivable; and they treat my statement that I cannot conceive that the image on my retina is upside down, as if I had said that I could conceive of the image if it were anything else than upside down.

If for purposes of illustration I declare my conviction that the moon is not made of green cheese, what are we to think of the 'logic' which interprets this as an assertion that it is made of cheese, although this is not green? I

can see no better logical warrant for attributing to me the opinion that I can conceive of the retinal image, but not of its inversion; for, most assuredly, I have said nothing of the sort, and I find all the physiological antecedents to vision equally inconceivable.

If something in the minds of certain writers leads them to believe that I adhere to an obsolete and worthless hypothesis of vision I am helpless, for while I have the right to demand that my words shall pass at their face value I have no way to defend this right except an appeal to unprejudiced readers.

I cannot conceive of the antipodes, and if C. L. F. infers that I accept the astronomy of Homer I must bear up as well as I can.

Both the rotundity of the earth and the inversion of the retinal image are proved by ample evidence, but apprehension of the proof of a truth is a very different thing from conception of the truth itself, and no one who is not totally destitute of imagination could confuse the one with the other; although it may be well to remind C. L. F. that I have nowhere said that 'there is anything which needs explanation in the fact that the image on the retina is inverted,' and that it is because the evidence is conclusive that I made use of the inversion to illustrate that great law of logic that '*the test of truth is evidence and not conceivability.*' (SCIENCE, Oct. 4, 1895.)

If any reader cares to ask what has called forth all this criticism, which has occupied the pages of SCIENCE for more than six months, he may be surprised to find that my statement about the retinal image was nothing more than an incidental illustration of less than a dozen words in an article in SCIENCE, October 4, 1895, in which I tried to show that "the mental vice to which we are most prone is our tendency to believe that lack of evidence for an opinion is a reason for believing something else."

The correspondence which this illustration has excited seems to show that I should have done well to state this truth in a more general form, and to point out that the mental vice to which we are most prone is our tendency to interpret a negation as an affirmation of something else.

W. K. BROOKS.

CERTITUDES AND ILLUSIONS.

TO THE EDITOR OF SCIENCE: In my first article on 'Certitudes and Illusions,' I cited two illustrious examples of persons who had lapsed into reification, namely, Spencer in his 'First Principles,' where he reifies force, and Hegel in his Logic where he reifies idea or comprehension; but I did not attempt to exhibit Spencer's reification of force or Hegel's reification of idea. In that article I tried to set forth the nature of the subject-matter of a series of articles which I had planned and promised the editor.

Fichte has seized upon certain of Kant's reifications and those of others and reasoned about non-existent abstractions or pure properties of mind, and in his presentation has naively reduced the whole method of reasoning to an absurdity; but he died a disappointed and sad man because he had not consciously discovered that he had murdered his own methods. Hegel seems to have discovered this and to have characterized pure abstraction in no unmeasured terms, notwithstanding which he finally fell into the same vice and reified idea. In my first article Hegel's illusion was not set forth, but only reference made to the matter for the purpose of calling attention to the subject-matter of which I wish to treat. I shall not ignore or underestimate Spencer's contribution to the biology of the lower animals nor his contribution to psychology. In the same manner I shall not underestimate Hegel's acute reasoning in his system of logic, but I shall attempt to show that Hegel accepts Kant's doctrine of antinomies and develops this doctrine into a logic of contradiction and by its use reifies idea and ends as an absolute idealist. Now, Mr. Editor, permit me to say this word in reply to Prof. Royce, whose letter is in every way kind, but whose error consists in supposing that I attributed to Hegel all of the reifications mentioned in my article.

If he will take down the *Phänomenologie des Geistes* and read in the first chapter what Hegel has said about the demonstratives, and then read what I have said about them, he will discover to what I had reference in the treatment and use of these demonstratives, and maybe he will further discover that I have a purpose in speak-

ing of the demonstratives, as I intend ultimately to develop certain doctrines of language most clearly brought out by them.

Since writing the above the managing editor of this journal has kindly forwarded the proof sheets of Prof. Fullerton's article, about which I beg to be indulged in a brief statement.

In my first paper it will be seen that I did not attempt to demonstrate anything; for I said: "In the following chapters an attempt will be made to show that we know much about matter, and although we do not know all, all we know is about matter in its categories of number, extension, motion, duration and judgment, or that we know of matter in its four categories and that we know of mind in the categories of judgment, but always this mind is associated with matter. In doing this we shall endeavor to discriminate between the certitudes and illusions current in human opinion."

I merely attempted to explain the nature of the problems which I designed to discuss and to show that these problems are fundamental to metaphysic and to science alike. To indicate that there are two views of these problems—the metaphysical view and the scientific view—I shall attempt to set forth a series of certitudes and another series of illusions which relate to these certitudes. If I prosper in my demonstration I shall show that the certitudes come from science and that the illusions come from metaphysic. Now it must be understood that metaphysic does not deal wholly with illusions but that fundamental illusions are developed by metaphysical reasoning, and I shall further show that science attempts to deal with certitudes, but often fails by adopting the method of metaphysic and still oftener adopts its illusions. The illusions which I shall attempt to explain will be chiefly illusions of metaphysic, but they will also be illusions of science, because science has not wholly divested itself of metaphysical reasoning. The certitudes which I shall attempt to demonstrate I shall hold myself ready to maintain until my errors are shown; if such errors are demonstrated I shall promptly confess and eschew. I do not know that the man who has published can fully assume this attitude, for in a long life of scientific reading I have discovered

that publication is wax in the ears and thus a source of profound deafness to the voice of reason. If Prof. Fullerton will kindly attend to the propositions I shall attempt to demonstrate, he will be able to put me right where I am wrong, and I hope that he will be able to reinforce my certitudes by firmer rings of reasoning.

Professor Fullerton seems to be surprised and agrieved that an anthropologist should express opinions concerning metaphysic. The Professor may be interested to know that anthropology includes metaphysic as one of its themes of study for the purpose of discovering its certitudes and illusions and it sometimes finds in its ancient asphodel fields phantom flowers that turn to ashes when plucked by the hand of science.

J. W. POWELL.

SCIENTIFIC LITERATURE.

Geological Biology; an introduction to the geological history of organisms. By HENRY SHALER WILLIAMS. New York, Henry Holt, 1895. xx+395, pp. 8°. Illustrated.

Prof. Williams tells us that this book was originally written in the form of lectures delivered at Cornell University, which have been rewritten and elaborated so as to be available for use as a text-book as well as an exposition of principles. It has been prepared with a view to its use not only by students, but also the general reader "who is supposed to know something of the present popular theories regarding organic life, and has, perhaps, already become aware of the increasing sense of disappointment which those are meeting who have attempted seriously to apply them to the solutions of the problems of human life." It is not assumed that the reader has any special knowledge of biology or geology, and therefore many details are entered upon which would be superfluous for the specialist. "In defining our topic as geological biology we are not proposing to investigate the anatomical organs and tissues of which particular animals are made, but to review the facts and theories which have led to the belief that each living animal and plant is but the last of a long line of organisms whose remains can be recognized in more or less perfect fossils and whose varying characters can be traced back into the

immense antiquity of geological time" (p. 3). "The history of organisms which we particularly trace in the study of fossils is not the history of imperfect organisms struggling toward perfection, but it is the history for each age and epoch of the perfected adjustment of the organisms of the time to the particular conditions of environment in which they lived. They did not die before their time, overcome by the mythical fittest who are said to survive in the struggle. They were the fittest and died natural deaths, having provided, before they gave up the struggle for their progeny, to succeed them. The hard parts record the history of adults which had endured the struggle, and thus represent the royal line of succession for the geological ages" (p. 81).

The book opens with a discussion of the history of organisms and its geological aspect. The second chapter gives an excellent and interesting summary of the history of geology, which is followed by a discussion of the geological time-scale, and of the nature, nomenclature and fossil contents of stratified rocks, geographical distribution, the nature and origin of species, the acquirement of characters, intrinsic and extrinsic, their plasticity and permanency. The rate of morphological differentiation and progressive modification are considered at length and illustrated by the history of selected types. The final chapters treat of the laws of evolution as illustrated by the geologic history of organisms and the philosophical conclusions drawn therefrom.

The author concludes that "the Animal Kingdom is divisible into a number of definite groups marked by definite organization, all the grander features of which were outlined in the Cambrian age, and the large majority of all the differentiations of even ordinal rank had been accomplished in the first quarter of the recorded history of organisms," hence the laws of evolutionary history must be read in terms of the minor groups. As emphasized by fossils these laws include an orderly succession of increasing differentiations in organic structure which we call evolution; certain parts of each organism exhibit the progress of evolution more rapidly than other parts, the characters of least structural importance showing the most constant

and steady but slow differentiation, while the characteristics of higher rank are relatively more rapid in their initial development and subsequently very constant in each successive generation. These two tendencies are expressive of the two fundamental laws of heredity and variability, and the process of evolution is the combined result of their interaction. The mode of evolution consists in the acquirement of new characters by variation and in the acceleration or retardation of development of characters already required. The causes of evolution are extrinsic or intrinsic, the former being of the nature of an adjustment to the environment direct or selective; the latter "acts previous to the individual birth and seems to be at the foundation of variability. The mode and manner of expression of this kind of evolution are more difficult to define than in the case of extrinsic evolution, but the facts of paleontology clearly indicate that such a cause exists prior to the morphological appearance of each individual and species" (pp. 369-70).

"The great facts attested by geology," according to Prof. Williams, "are that the grander and more radical divergencies of structure were earliest attained; that, as time has advanced, in each line intrinsic evolution has been confined to the acquirement of less and less important characters; such facts emphasize with overwhelming force the conclusion that the march of the evolution has been the expression of a general law of organic nature in which events have occurred in regular order, with a beginning, a normal order of succession, a limit to each stage, and in which the whole organic kingdom has been mutually correlated. * * * So were we to lengthen out the gyration of organic plastidules or biophores, a million million years, continuously holding on to their original powers and potencies for all that time, we are not relieved in the least from the logical necessity of endowing them at the outset with the real directive energy which phenomenally expresses itself for the first time when the finally adjusted organism appears. And the increment to organic structure expressed by their final bursting into morphological reality after travelling unobserved but potential through the organic matter of countless generations is as

much a result of creative energy as if a new species were to arise out of the dust of the earth" (pp. 380-382).

It is of course almost impracticable by means of isolated paragraphs to give any adequate impression of a whole volume of observation and discussion with a wealth of varied illustration. But we shall not go far astray, perhaps, in summing up Prof. Williams' attractive book as in great part a restatement, in terms of evolution, of the argument for design in nature.

W. H. D.

WINGE ON BRAZILIAN CARNIVORA.

In a recently published quarto of 103 pages * Mr. Herluf Winge gives the results of his studies of the extensive collections of Carnivora made near Lagoa Santa, province of Minas Geraes, southeastern Brazil, by Lund, Reinhardt and Warming, and now in the Zoölogical Museum at Copenhagen. The material thus brought together owes its peculiar interest to the fact that it consists partly of the remains of living animals and partly of bones and teeth from the earth deposits of the caves with which the region abounds. It is thus possible to compare the present fauna with the extinct fauna of which it is the immediate successor. As the author remarks (p. 79), the South American fauna is poorer in Carnivora than that of any other region except Australia. The latter was, however, probably isolated before the appearance of the order. While Lagoa Santa is, for a South American locality, remarkably well provided with Carnivora,† the group is represented by only four families, ten genera and twenty-five species. These the author arranges as follows:

* Jordfundne og nulevende Rovdyr (Carnivora) fra Lagoa Santa, Minas Geraes, Brasilien. Med Udsigt over Rovdyrenes indbyrdes Slægtskab. Af Herluf Winge. Aftryk af 'E. Museo Lundii,' en Samling af Afhandlinger om de i Brasiliens Knoglehuler af Professor Dr. P. W. Lund udgravede Dyr og Menne-skeknogler. Paa Carlsbergfondets Bekostning ud-givet ved Professor Dr. C. F. Lütken, Kjöbenhavn, 1895.

† *Bassaricyon*, *Cerculeptes*, *Lyncodon* and *Mustela* are the only genera, except perhaps a few now extinct, known to occur in South America, that have not yet been detected there.

FELIDÆ: *Felis tigrina*, *F. macroura*, *F. eira*, *F. concolor*, *F. onca*, *Machærodus neogæus*.

URSIDÆ: *Canis azaræ*, *C. vetulus*, *C. cancrivorus*, *C. jubatus*, *C. troglodytes*, *Icticyon pacivorus*, *I. venaticus*, *Ursus brasiliensis*, *U. bonariensis*.

PROCYONIDÆ: *Nasua narica*, *Procyon ursinus*, *P. cancrivorus*.

MUSTELIDÆ: *Galictis barbara*, *G. intermedia* (= *G. allamandi*), *G. vittata*, *Thiosmus suffocans* (= *Conepatus mapurito*), *Lutra platensis* (= *L. paranensis*), *L. brasiliensis*.

Twenty-three of these are found in the cave deposits ('jordfundne'), while eighteen are found living in the vicinity ('nulevende'). Two species, *Procyon cancrivorus* and *Lutra brasiliensis*, now occurring near Lagoa Santa, have not yet been detected among the cave remains. As the author remarks, however, this can scarcely be taken as evidence that the animals have recently appeared in the region. Among the Carnivora whose remains are found in the caves are six extinct species, and one, *Canis azaræ*, which though now widely distributed through South America, has not yet been taken at Lagoa Santa. The extinct species are *Machærodus neogæus*, *Canis troglodytes*, *Icticyon pacivorus*, *Ursus brasiliensis*, *U. bonariensis* and *Procyon ursinus*. *Machærodus neogæus* is one of the most highly developed as well as one of the largest members of its genus. It is also one of those which have most recently become extinct. The Copenhagen museum contains numerous remains of this animal from La Plata. These, however, do not differ from the Lagoa Santa bones in any essential way. The two closely related bears, *Ursus brasiliensis* and *U. bonariensis*, are in some respects more primitive in structure than other species of *Ursus*. They form, together with *Ursus simus*, a section or subgenus which is extinct, and as yet is known from South America and California only.*

Icticyon pacivorus is closely related to the recent *I. venaticus*. It is more primitive than the latter, of which it appears to be the direct ancestor. *Canis troglodytes*, also one of the extinct species, has much the same general form as the Old World *C. alpinus*. A detailed study of its

*See Cope, American Naturalist, XIII., p. 791, 1879, and *ibid.*, XXV., p. 997-999, pl. XXI., 1891.

characters show, however, that it is in no way closely related to *C. alpinus*, but, on the contrary, is a special offshoot from some South American dog of the ordinary type. *Procyon ursinus*, while showing certain characters which prove it to stand nearer the ancestral stock than do the existing species, is considerably larger than any of the latter.

Turning now to the living Carnivora, Mr. Winge gives very extended and elaborate discussions of the specific characters and of the individual variation in both size and color of most of the forms represented in the collection. While the author's tendency to reduce the number of species to the minimum must detract from the critical value of this part of the work, the facts recorded will be of the utmost use to all workers on South American mammals. The discussions of the species of *Felis* and *Canis* are especially important.

For the most part the bones found in the caves agree perfectly with those of the living representatives of the various species. There are, however, a few exceptions to this rule. Thus, only a few of the cave remains of *Felis onca* are of the same size as those of the ordinary existing Jaguar. Most of them represent animals which were about the size of *F. tigris*. There is scarcely any difference in the teeth, but in the bones the discrepancy is very noticeable. Although the cave Jaguars average much larger than those now living, one of the latter occasionally fully equals the largest of the former. The single perfect skull of *Icticyon venaticus* from Lapa dos Tatus differs remarkably from recent skulls of the same species. The nasal bones are much more produced both before and behind, while the whole skull is larger; the rostrum is considerably broader in proportion to the brain case, and the zygomatic arches are more flaring posteriorly. If the differences between this skull and that of the recent specimen as figured on plate V. are in no way due to age and sex, few mammalogists would hesitate to separate the animals specifically. Mr. Winge, however, does not consider such a course advisable, though he admits that the Lapa dos Tatus skull may represent a 'geological race' ('men maaske er det en geologisk race').

As the result of his studies of the interrelationships of the Carnivora in general, Mr. Winge gives the following table of super-generic groups (p. 46, 47).

- Carnivora primitiva.
 - Hyænodontidæ.
 - Proviverrini.
 - Mesonychini.
 - Hyænodontini.
 - Arctocydonidæ.
- Carnivora vera.
 - Herpestoidei.
 - Amphictidæ.
 - Palæonictidæ.
 - Felidæ.
 - Felini.
 - Machærodontini.
 - Viverridæ.
 - Viverrini.
 - Herpestini.
 - Hyænidæ.
 - Arctoidei.
 - Ursidæ.
 - Canini.
 - Ursini.
 - Procyonidæ.
 - Mustelidæ.
 - Mustelini.
 - Melini.
 - Lutrine.
 - Otariidæ.
 - Trichechini.
 - Otariini.
 - Phocidæ.

This arrangement differs in many details from that recently adopted by Flower and Lydekker.* The latter authors divide the order into fifteen families and one hundred and six genera, while Winge recognizes the same number of genera and only twelve families. Perhaps the most noticeable peculiarity of the present classification is the treatment of the bears, dogs and raccoons. The two former, or the families *Ursidæ* and *Canidæ* of Flower and Lydekker and of Zittel,† are here treated as subfamilies of the family *Ursidæ*, while the *Procyonidæ* are kept distinct. *Canis* and *Icticyon* are thus brought close to *Ursus*, while *Procyon* is placed in a different group.

* Mammals Living and Extinct, 1891.

† Handb. der Palæontologie, Mammalia, 1892-1893.

In matters of nomenclature, Mr. Winge, as usual, disregards the chief laws through the strict application of which stability of names is alone to be reached. He either believes that uniformity in the use of generic and specific names will best be brought about by allowing the individual preference of each writer full play, or else he takes the less optimistic ground that such uniformity is unattainable and therefore not worth striving for. Be this as it may, a casual examination of the names used in the present paper shows that the author prefers *Galictis intermedia* Lund 1843 to *G. allamandi* Bell 1841, *Lutra platensis* Waterhouse 1839 to *L. paranesis* Rengger 1830, *Thiosmus* Lichtenstein 1838 to *Conepatus* Gray 1837, *Rhizena* Illiger 1811 to *Suricata* Desmarest 1804, *Mydaon* Gloger 1841 to *Mydaus* Cuvier 1821. He also uses the untenable names *Bassariscus*, *Elurogale* and *Enhydrius*, although they have been replaced by *Bassariscus*, *Ailurictis* and *Latax*, respectively. It is difficult to understand why *Trichechus* Linnæus 1758 based on the Florida Manatee should be preferred to *Odobenus* Brisson 1762 as the generic name for the Walrus. Yet *Trichechus* and its derivative *Trichechini* are both adopted by Mr. Winge.

Like the earlier papers of this series* the present work is divided into three main parts: (1) nominal lists of the species; (2) detailed accounts of the species with critical notes on their relationship; (3) a review of the mutual inter-relationships of the members of the group at large. To the present paper is appended a table showing semi-graphically the changes that take place in the fifth, fourth, third and second of the original seven cheek teeth throughout the genera of Carnivora (p. 100-103).

The paper is illustrated by eight plates from photographs of specimens. Although the results obtained by photographic processes are not yet sufficiently uniform to meet all requirements the figures are in general satisfactory, especially some of those on plates three, five and eight.

GERRIT S. MILLER, JR.

*Mr. Winge has already published in 'E. Museo Lundii' accounts of the rodents, bats, marsupials and monkeys of Lagoa Santa. His paper on the monkeys was noticed in SCIENCE, N. S., II., No. 50, December 13, 1895.

Ethnology, By A. H. KEANE, F. R. G. S. 1 vol. 8vo. Illustrated. Pp. 442. Cambridge University Press. 1896. Macmillan & Co., New York.

The above work is one of the 'Cambridge Geographical Series' published under the general editorship of Dr. F. H. Guillemard. The author takes 'ethnology' in its ancient and now generally obsolete sense, nearly synonymous with 'anthropology,' as employed in modern science. Following this definition, he divides his volume into two parts, 'fundamental ethnical problems' and 'primary ethnical groups.' Under the former he discusses such questions as man's place in the animal kingdom, tertiary and quaternary man, the growth of mind and the study of the brain in relation to thought, the antiquity of the human race, the palæolithic and neolithic ages, the theories of polygeny and monogeny, the physical and mental differences of races, their languages and social regulations.

Under the second heading the author's theory of races or groups is presented. It is a modern recast of that of Blumenbach, retaining even his inappropriate term 'Caucasian' for the white race. The other are the Ethiopian, Mongolian and American races; the Malayan race being explained away as partly Ethiopic, partly Caucasian. Of these he undertakes to give the divisions and subdivisions from such authorities as he has consulted.

The manner in which this task has been accomplished will give satisfaction to the general reader. Many questions which the student of the science must consider as still pending, Mr. Keane disposes of with a magisterial decision. He rarely presents the opposing evidence in its proper strength, and refers to those with whom he disagrees as 'eccentric,' or 'reckless,' or "extravagant," or by other disparaging adjectives. He does not hesitate to strain a point to defend his opinion (e. g., p. 34, Virchow's judgment of the Neanderthal skull), and, it would seem, cannot certainly have remembered some of the authors whom he quotes, or he would not claim as original with himself (p. xiv.) such theories as the local evolution of American cultures, the peopling of America from both Europe and Asia, the relationship of Basques

and Berbers, etc. Whether true or not, these are certainly not new views to one acquainted with the current literature of the science.

The relationship of the members of the various races is shown by 'family trees,' an ancient and necessarily misleading device. Thus, his tree of the 'homo Caucasicus' puts the Greeks, Celts and Etruscans on one of its primary branches, along with Circassians and Dravidas, while the Teutons and Slavs are on a different branch! That on this tree are placed the Samoans, Hawaiians, Battaks and Khmer is to be explained by the author's theory of the Malayan race above referred to, which he claims and which we may allow is at present, and is likely to be, his own peculiar property.

The tree of the 'homo Americanus' becomes a mass of inconsistencies so soon as he leaves the protection of Major Powell's linguistic map. Even within its area the Kolosch and Selish are depicted as proceeding from the Eskimo! The chapter on the American race is replete with positive assertions, nearly always unsupported, for instance, the imaginary distribution of two types of skull (p. 362), the alleged impassiveness of the native character (p. 353), the 'undoubted' approximation of the American to the Mongol type, etc. It is obvious that the author has not consulted the best and most recent studies in American aboriginal ethnography; yet his chapter might have been much more un instructive than it is.

The proof-reading is generally satisfactory, though probably a highly respected American writer will not think so when he sees himself referred to as 'Mr. Thomas Cyrus' (p. 370).

Of ethnology proper, in the sense in which it is now adopted by the leading German, French and American writers, the volume scarcely treats at all, and we may look in the Index in vain for the names of Bastian, Post, Steinmetz, Achelis, or the other distinguished representatives of that comparatively new and grand department of learning; and while Mr. Keane's book can be recommended as an industrious compilation, useful to public libraries and well put together, the warning should be distinctly uttered that its title is an error and that it bears scarcely at all on the science of ethnology.

D. G. BRINTON.

Las rocas eruptivas del suroeste de la cuenca de México. EZEQUIEL ORDOÑEZ. Boletín del Instituto Geológico de México. No. 2. México, 1895. Pp. 46.

The contents of the first bulletin of this series were briefly noted in SCIENCE, Vol. II., pp. 739-740. In this issue, Señor Ordoñez presents a very clear description of the important volcanic district of the valley of Mexico, and particularly of the volcanic group of Santa Catarina and of the volcanic rocks of the Sierra de las Cruces. Fourteen opening pages are devoted to giving an 'Idea general de la cuenca de Mexico.' The remainder of the paper, largely petrographic, gives a detailed account of the cones, lava flows, breccias and ashbeds of the southwestern part of this region of andesites, trachytes and intermediate petrographic types.

J. B. WOODWORTH.

SCIENTIFIC JOURNALS.

THE ASTROPHYSICAL JOURNAL, FEBRUARY.

THE leading article is by Prof. L. E. Jewell, upon the coincidence of solar and metallic lines and upon the appearance of lines in the spectra of the electric arc and the sun. When compared with corresponding solar lines, the metallic lines of the arc spectra have been found to be almost invariably displaced toward the violet. There is, moreover, a difference in the amount of displacement of lines belonging to the same element; the greatest shift being observed in the strongest lines.

An explanation for this was sought in the difference between the condition of matter in the arc and in the solar atmosphere. This difference is probably that of pressure or density of material and temperature, or both.

The lines least displaced were those not easily reversed and visible only at a high temperature or when a large amount of material was used. As the solar lines agree most nearly with the lines produced in the center of the arc, where the temperature and density are high, we have the means of determining the pressure or the temperature of the solar atmosphere, where the Fraunhofer lines are produced, if we can separate the effects of temperature and pressure.

Several lines of investigation lead to the con-

clusion that the wave-length is affected little, if any, by changes of temperature.

The effect of pressure and amount of material used, is discussed in the next article by W. J. Humphreys and J. F. Mohler. Their results corroborate those of former investigators, that an increase of the quantity of material in the arc generally produces a slightly unsymmetrical broadening towards the red, and that increased pressure causes a similar effect; but they find in addition that, upon the application of pressure, a decided shift of the lines toward the less refrangible portion of the spectrum takes place, not due to broadening.

The change in the case of any one element is approximately proportional to the wave-length and to the excess of pressure above one atmosphere. Whether the same law holds for very low pressures could not be determined. The shift for different metals at a pressure of fourteen atmospheres often amounted to from five to ten hundredths of an Ångström unit.

Theoretical considerations indicate that the shift is inversely proportional to the absolute temperature of the melting point. There is, besides, a connection between the shifts and the atomic weights.

A third paper, by the authors of the first two, contains a discussion of the effect of pressure upon the reversing layer of the solar atmosphere. Assuming the atmosphere to be quiescent and the shifts to be due to change of pressure only, they find the pressure of the reversing layer to vary from two to seven atmospheres, according to the weight of the element observed. This seems to indicate that the upper limits of the reversing layers of the different elements are arranged somewhat in the order of their atomic weights.

THE AMERICAN GEOLOGIST, MARCH.

Biographical Sketch of Charles Wachsmuth: By C. R. KEYES. Dr. Wachsmuth was known as the foremost authority on fossil crinoids in America, if not in the world. He spent most of his life in Burlington, Iowa, where he first acquired an interest in crinoids and where he died on February 7, 1896, in his sixty-seventh year. The sketch is accompanied by a portrait.

The Structure of Certain Paleozoic Barnacles:

By J. M. CLARKE. The genus of fossil barnacles called *Lepidocoleus*, which occurs in the lower and upper Silurian and lower Devonian, is shown to be a primitive and unmodified type of cirripede structure, consisting of but two vertical ranges of plates, both series terminating in a single plate which is axial and caudal. The dorsal margin of the body is closed by the interlocking of the plates, but the ventral margin is closed only by their apposition and was dehiscent for the protrusion of the appendages.

The Mineral Deposits of Eastern California: By H. W. FAIRBANKS. The ore deposits discussed are chiefly those of gold and silver, the occurrence of the former being treated in considerable detail. The gold-bearing quartz veins are considered as unquestionably of fissure origin, and the contents of these veins bear no particular relation to the mineral composition of the country rock.

Note on the Discovery of a Sessile Conularia—Article I: By R. RUEDEMANN. The author has found in the lowest part of the Utica shale some specimens of *Conularia gracilis* Hall, to which are attached peculiar cuneiform fossils, and he has also found similar forms on two specimens of the shell of *Trochonema*. Evidence is presented to show that these peculiar forms are the young of *C. gracilis*, as is also the so-called plant, *Sphenothallus angustifolius* Hall. These young individuals of *Conularia* were not free but attached, and it is probable that the adult forms were also sessile, although attached adult specimens have not yet been found.

A New Titanichthys: By E. W. CLAYPOLE. The new form is much smaller than the other species of the genus and is named *Titanichthys brevis*. It was found by Dr. Clark, who has discovered so many fossil fish in the Devonian of Ohio.

Thickness of the Paleozoic Rocks in the Mississippi Basin: By C. R. KEYES. The estimate made by various geologists of the Paleozoic rocks of the Mississippi basin are given, and attention is called to the fact that the estimates of later years are considerably lower than the others. A recent deep boring at Kansas City, which is in the region of greatest thickness of the Paleozoic, has passed through the entire (with the exception of a part of the upper Coal meas-

ures) Paleozoic column, and entered mica schist, which is regarded as Archean in age. Estimates from this boring give about 3,000 feet as the total thickness of the Paleozoic.

Microscopic Characters of the Fisher Meteorite (Minnesota No. 1): By N. H. WINCHELL. This meteorite fell in Polk County, in northwestern Minnesota, April 9, 1894. It is a chondritic stone made up largely of olivine and enstatite, and contains a comparatively small amount of iron. Two apparently isotropic substances occur in the meteorite, one of which may be maskelynite; but the conclusions concerning these substances and the chemical composition of the stone will be discussed in a later paper.

The number closes with the usual reviews and notes. Of special interest, however, is the review of Nordenskjöld's important paper on the Swedish hälleflintas, largely pre-Cambrian lavas. In this review are some interesting remarks concerning the devitrification of glass, and reference is made to some phenomena of devitrification recently observed at Bryn Mawr College.

SOCIETIES AND ACADEMIES.

PHILOSOPHICAL SOCIETY OF WASHINGTON, FEBRUARY 29, 1896.

DR. J. WALTER FEWKES read a communication on the Prehistoric Culture of Tusayan. He regarded archaeology as the only means of obtaining accurate knowledge in regard to the subject, and considered documentary history, study of surviving legends and modern practices as tributary and necessary sources of information. Archaeological evidences of the character of ancient life in Tusayan were drawn from excavations at Sikyatki, a ruined pueblo near Walpi, which was investigated by an expedition sent out last summer by the Smithsonian Institution under the lead of the speaker. The material unearthed from this ruin was a large collection of pottery of rare excellence and many objects illustrative of prehistoric Tusayan industries.

The evidences that Sikyatki was overthrown previous to the coming of the Spaniards into Tusayan in the middle of the sixteenth century were discussed, and shown to amply prove that the pueblo was destroyed in prehistoric times.

The great value of the objects from this ruin was therefore held to be that they indicate prehistoric culture, without European influences. The ceramics of Sikyatki are far superior to modern Tusayan pottery, and excel in fineness of ware, symmetry of form and artistic beauty of decoration those of any aboriginal tribe of America north of Mexico.

The reason of the fineness of this ware and the possibility that coal was used in firing it were discussed. The identity of prehistoric and modern mortuary customs, as indicated by the objects taken from Sikyatki graves, was interpreted to mean a similarity of ancient conceptions of death and a future life. Current modern beliefs on this subject were discussed and applied to an interpretation of ancient customs.

It was held by the speaker that the symbolic designs on this ancient pottery should be considered a body of prehistoric picture writing or paleography, and that the aim of the student should be to interpret it. He likened this symbolism to ancient records, and claimed that from them could be obtained a knowledge of mythological conceptions and ancestral rituals. The pictures of several animistic and other gods still recognized in modern Tusayan mythology were instanced and compared with modern figures. This ancient pictography likewise shows the antiquity of peculiar methods of dressing the hair.

The resemblance of certain geometric designs to those on the pottery from the great ruins of the Gila valley and the cliff dwellings of the Mesa Verde was pointed out and the importance of such likeness discussed.

Dr. Fewkes spoke of a large number of mortuary prayer-sticks or pahos in Sikyatki graves, which he compared with modern and found a great conservatism in their form, size, color and appendages. He believed these resemblances meant a similarity in ancient and modern conceptions of the priests who made them. The existence of other ceremonial paraphernalia, identical with those still used in the modern Tusayan ritual, was likewise pointed out. A knowledge of modern mythology and ritual he regarded as necessary for anyone who would do good work on the archaeology of the Southwest.

He showed that in prehistoric times the Tusayan people had Oliva shells from the Pacific Ocean and turquoises from New Mexico. They were ignorant of any metal, but were adepts in stone chipping and polishing. Fabrics were made from the feathers of the bluebird and eagle, and they had necklaces of cedar berries, turkey bones, with ornaments of lignite, selenite and mica.

He brought forward additional evidence to show the identity of cliff dwellers and ancient pueblos and considered that some of the cliff houses were inhabited when Sikyatki was in its prime. In closing Dr. Fewkes emphasized the poverty of material in museums from which we could draw evidences for speculations in regard to the derivation of prehistoric pueblo culture, and held that theorizing had far outstripped observation. While considering science piteously weak in data, he thought that no field offered more promising results to a serious student than the ruins of the Southwest.

The second paper was on *a new solution of the geodetic problem*, by CHAS. H. KUMMELL, of U. S. Coast and Geodetic Survey. This solution is based on the geodetic line. It requires therefore a reduction of the astronomical azimuth to the geodetic azimuth and, theoretically at least, a reduction of the astronomical latitude to the reduced latitude. An auxiliary spherical triangle is assumed, having the equation of the geodetic line for its sine relation, which is referred to that point as origin where it meets a meridian at right angles. The distance in arc of the first point from that meridian is denoted δ , that of the second $\sigma_2 = \sigma_1 + \Delta\sigma$. The arc σ_1 is easily computed, and $\Delta\sigma$ is found from a series of which only three terms are required even for the greatest intervisible distances. We have now in the auxiliary spherical triangle the sides $\Delta\sigma$, $90^\circ - \psi_1$ (complement of reduced latitude of first point), and included angle a_1 (geodetic azimuth to second point). We can find then by rigorous spherical trigonometry the parts a_2 and $90^\circ - \psi_2$ and hence also the astronomical back azimuth a_2 and latitude ϕ_2 . To attain the customary precision this would require ten-place logarithms. In order to reach the same accuracy with seven place logarithms formulæ are given for computing the

convergence of meridians $\Delta\alpha$ and difference of latitudes $\Delta\phi$. For $\Delta\lambda$, the difference of longitudes, two methods are given, one based on the geodetic line by computing $\Delta\lambda_0$, the angle opposite the side $\Delta\sigma$ and correcting this, by a term of the fourth order in eccentricity and first order in distance. The other method is by Dalby's theorem and is more convenient. A complete example computing the position of Königsberg from Berlin was exhibited, which showed the formulæ used as precise as the ten-place computation of the same example in Helmert's *Hoehere Geodæsie*. The method is claimed to be principally advantageous for the greatest intervisible distances for which e and $\Delta\sigma$ are nearly of the same order (they are equal at about 500,000^m). For secondary points $\Delta\sigma$ is much smaller than e and in that case Tables and Formulæ such as Woodward's Smithsonian Geographical Tables and those of the Coast Survey which go an order higher are preferable.

BERNARD R. GREEN,

Secretary.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

THE 115th regular meeting was held March 5, 1896.

Mr. Marlatt, under the title 'A Study of the Anatomy of Hymenoptera,' gave a comprehensive view of certain structural features of Tenthredinidæ, dwelling at length upon the homologies of the sclerites of the thorax.

Mr. Schwarz, under the head 'Notes From Southwestern Texas, No. 2,' spoke of a species of Termite which is found in great numbers throughout southwestern Texas, which burrows deeply under the ground and which is of great economic importance from the fact that during a large part of the summer it destroys all low-growing vegetation in large patches, rising from the ground and enclosing all portions of the plants with a tubular structure composed of grains of subsoil. The insect is probably the worst insect pest of southwestern Texas, on account of the damage which it does to pasture.

Mr. Ashmead exhibited a specimen of a new species of *Roctronia* of Provancher. The species in question comes from California, and by its aid Mr. Ashmead has decided that this genus be-

longs to the subfamily Helorinæ of the Proctotrypidæ.

L. O. HOWARD,
Recording Secretary.

NEW YORK ACADEMY OF SCIENCES—BIOLOGICAL
SECTION, FEBRUARY 7, 1896.

DR. J. G. CURTIS in the Chair.

A communication from the Council was received asking that the Section take action on Representative Hurley's bill "To fix the standard of weights and measures by the adoption of the metric system of weights and measures."

On motion of Dr. Dean the Section approved the bill, and the Secretary was directed to express the entire commendation of it to the Council.

Dr. Arnold Graf read a paper on 'The Structure of the Nephridiap in Clepsine.' He finds in the cells of the intra-cellular duct fine cytoplasmic anastomosing threads which form a contractile mechanism. These are stimulated by granules which are most numerous near the lumen of the cell, and thus a peristalsis is set up, which moves the urine out of the duct. In the upper part of the intra-cellular duct the two or three cells next to the vesicle or funnel have no distinct lumen, but are vacuolated; the vacuoles of the first cell being small, those of the second larger, and so on, till the vacuoles become permanent as a lumen. He explains the action of the first cell as being similar to the ingestion of particles by the infusorians. The matter taken up thus from the funnel by the first cell is carried by the rest, and so on till the cells having a lumen are reached. The presence of the excrement causes the granules to stimulate the muscular fibres of the cells; peristalsis results and the substance is carried outwards. The character of this contractile reticulum offers an explanation of the structure of a cilium as being the continuation of a contractile reticular thread.

N. R. Harrington, in 'Observations on the lime gland of the Earthworm,' described the minute structure of these glands in *L. terrestris*, and showed that the lime is taken up from the blood by wandering connective tissue cells which form club-shaped projections on the lamellæ of the gland, and which pass off when filled with lime. The new cell comes up from

the base of the older cell and repeats the process. This explanation is in harmony with the fact that in all other invertebrates lime is laid down by connective tissue cells. Histological structure and the developmental history confirm it.

Dr. Bashford Dean offered some observations on 'Instinct in some of the lower Vertebrates.' The young of *Amia calva*, the dogfish of the Western States, attach themselves, when newly hatched, to the water plants at the bottom of the nest which the male *Amia* has built. They remain thus attached until the yolk sac is absorbed. As soon as they are fitted to get food they flock together in a dense cluster following the male. When hatched in an aquarium they go through the same processes. The young fry take food particles only when the particles are in motion, never when they are still. The larvæ of *Necturus* also take food particles that are in motion.

C. L. BRISTOL,
Secretary.

NEW YORK ACADEMY OF SCIENCES.

AT the meeting of the Section of Astronomy and Physics held on March 2, 1896, the election of officers for the ensuing year was held, and R. S. Woodward was elected Chairman and W. Hallock Secretary.

The first paper of the evening was upon the device designed by Prof. W. L. Robb for showing the way in which a cord can vibrate, consisting essentially of an electro-magnet running a vibrating arm to the end of which the string is attached.

The second paper was upon a new form of polariscope, designed by Prof. A. M. Mayer, consisting of a special arrangement of crossed lenses, resulting in unusually good illumination and large field.

The next paper was upon a heliostat designed by Prof. Mayer. In this connection Prof. Mayer called attention to the shortcomings of the various forms of heliostats, and especially those using only one mirror, pointing out, among other things, the useless width of mirrors on such heliostats, and illustrating what ought to be the dimensions of such a mirror. He also called attention to the great advantage of using sunlight for all optical experiments over any

form of electric light; for example, with a heliostat and condensing system he was able to project the interference bands of the Fresnel bi-prism upon a screen so that they were visible across a large room. Prof. Mayer's heliostat consists in a clockwork driving a shaft parallel to the earth's axis; upon the southern end of the shaft is the mirror that can either be twisted on the shaft or set at any angle to the shaft. The second mirror is mounted upon the base with its central point in the prolongation of the shaft. To orient the heliostat it is only necessary to bring the side pieces in the north and south line and then set the mirror on the clock axis. This last is done by covering the elliptical mirror with a paper having a $\frac{1}{2}$ -inch hole in the center, and adjusting the tilting mirror until the small beam of light reflected from the mirror through the hole falls upon the center of the mirror attached to the base of the instrument; then starting the clock, the instrument will keep the beam in a constant direction. Prof. Hallock in discussing the paper called attention to the accuracy with which the heliostat operated, and related his experience with a very large one-mirror heliostat in the Smithsonian Institution at Washington, which, however, was thoroughly unsatisfactory. Prof. Woodward and Prof. Jacobi also entered into the discussion of the relative merits of the various heliostats, especially the typical one-mirror and two-mirror heliostats.

Prof. M. I. Pupin then brought before the academy some recent observations he had made while experimenting with X-rays. In the first place, he pointed out that certain Crooke's tubes after a certain amount of use had their vacuum improved, so that the induction spark passed outside the tube rather than through the tube. Prof. Pupin was at a loss to altogether explain the cause, but believed that it might be due to the condensation of some of the gas remaining in the tube, and explained several experiments which he had already made confirming the observation that the vacuum was improved with use, and that in proportion as the vacuum improved the tubes were better for X-ray photography. Another phenomenon observed by him was that in developing the photographic plates the development began at the glass side

of the film, leading to the inference that the X-rays penetrated the film and rendered the glass fluorescent, this fluorescent light then acting upon the film. Following the suggestion of this observation he painted the inside of a box with the platinum-barium-cyanide and laid a photographic plate against it, making a photograph then through the side of the box. The X-rays develop the fluorescence in the cyanide which fluorescent light affects the plate. In this way he obtained very good results with much shorter exposures than by the original method.

W. HALLOCK,
Secretary of Section.

BOSTON SOCIETY OF NATURAL HISTORY.

A GENERAL meeting was held February 19th, thirty persons present. Mr. A. W. Grabau showed a specimen of the broad variety of *Paradoxides harlani* Green from a third locality in South Braintree.

Prof. A. Hyatt called attention to several shadowgraphs taken in Germany by Mr. R. W. Wood. One of them shows plainly the bones, the position and outline of the lungs, heart and cesophagus of a mouse, and indicates the possibilities of the ray as an aid to natural history studies.

Mr. Outram Bangs read a paper on the terrapin (*Malaclemys terrapin* Schœff) as an inhabitant of Massachusetts. This species has been known for fifteen years as occurring in the creeks and salt marshes of Buzzard's Bay. It was formerly very abundant, but has lately become quite scarce. A comparison of the Buzzard's Bay material with a series from the Atlantic coast from Washington to Florida and from Mobile shows variations in color, marking, roughness of the shell, and in the size and shape of the skull. These variations, however, are not considered sufficient to form a separate race. The evidence that the terrapin is not native to Buzzard's Bay, but was introduced, was considered insufficient.

Dr. Joseph Lincoln Goodale spoke on the vocal sounds of animals and the mechanism of their production. He described the simplest type of larynx; also the four principal types, mentioning the best examples of each type. The three characteristics of sound were noted,

and the production and development of voice in man and in mammals described. The vocal cords and the glottis in birds were described, and the control, regulation and volume of voice mentioned.

Mr. C. J. Maynard, in commenting on Dr. Goodale's remarks, described the tympaniform membrane of birds, and mentioned that in the wild goose the whole bronchial tube formed one vibrating membrane. SAMUEL HENSHAW,

Secretary.

ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, MARCH 3, 1896.

MESSRS. MORRIS E. LEEDS and J. S. Stokes on behalf of Messrs. Queen & Co. made communications on the historical development of studies in connection with Röntgen photography, presenting the most advanced views regarding the nature of the X-rays as published by various investigators. They also exhibited a series of fine pictures illustrating the application of the process to the study of biology and the results obtained by the use of quick and slow plates and various developers.

Dr. Egbert having alluded to the results obtained by him from the direct rays of the sun through platinum plates, Mr. Leeds called attention to the desirability of experimenting with the sun's rays reflected from a mirror. If a positive result be obtained it would demonstrate either that it is incorrect to say that the rays cannot be reflected, or those producing Dr. Egbert's effects are not Röntgen rays.

Mr. Joseph Willcox presented a collection of 308 recent and fossil Fulgurs from various localities and geological horizons, illustrating with extraordinary completeness the evolution of the form.

A preliminary announcement was made of the presentation, by Dr. A. Donaldson Smith, of fine collections of mammals, birds, reptiles and insects made by him during his recent exploration of western Somali Land, Africa.

EDW. J. NOLAN,

Recording Secretary.

NORTHWESTERN UNIVERSITY SCIENCE CLUB.

At the meeting of February 7th Dr. Marcy in chair and thirty-three persons present, Dr. W.

A. Phillips presented a study of flaking refuse, based upon an extensive series of flakes and flaked cobblestones from sites of working, near Benton, Lake county, Illinois. The series offers several hundred outer flakes of which over two hundred are used flakes, assignable to six distinct uses from the character of the wear at edge or surface. Outer flakes are greatly in excess of other flakes in the refuse.

A large number of flaked cobblestones and of unused flakes, smaller, but still from the outside of the stone were treated as waste, nuclei and failures respectively. Specializations of the flake for hafting principally are represented, while further shaping of the cobblestone is wanting in a finished product. It is, however, represented in a limited series of rejects, indicating sporadic use of the nuclei. The rocks here used were diabase, found in the beach gravels of Lake Michigan, near sites. The nature and form of flake was due to the shape of the cobblestone. The operation used in producing the flake and illustrated by experimental results was referred directly to the hammerstone; the stone in the hand yielding the flake, the stone struck resting on the ground and serving only for the necessary percussion. A large number of lantern slides were used in illustration. Microscopic sections of rock from which the flakes came, prepared by Mr. Stebbins under Prof. Crook's direction, were exhibited.

A. R. CROOK,

Secretary.

EVANSTON, ILL.

THE ACADEMY OF SCIENCE OF ST. LOUIS.

At the meeting of March 2, 22 persons present, Mr. F. W. Duenckel presented a comparison of the records of the United States Meteorological Observatory, located on the Government building in the city, with the record for the Forest Park station, showing that the daily minimum averaged decidedly lower at the Forest Park station than in the city, while the wind averaged decidedly higher for the city station.

Prof. E. A. Engler spoke on the summation of certain series of numbers.

WILLIAM TRELEASE,

Recording Secretary.